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WUNDT'S PSYCHOLOGY

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This year signalizes the hundredth anniversary of Wundt's birth. Yet it is probably still true that, as Titchener wrote soon after Wundt's death in 1920, "we stand too near to Wundt to see him in a just perspective."[1] Clear vision, however, requires more than a proper perspective; with increasing distance, outlines are likely to blur. While, then, the time for a just appraisal of Wundt is not yet, certain preliminary studies can and need to be carried out now.

Wundt's was a long life, and his works were many. In his formative period, moreover, which reached to his forty-second year when he published the first edition of the *Grundzüge der physiologischen Psychologie* (1874), he bore the marks of many influences: Leibnitz, Kant, and Herbart; Helmholtz, Fechner, and Darwin. From 1874 to 1920, on the other hand, while he added greatly to the fields of experimental and social psychology, his psychological outlook persisted, through its many restatements, fairly unchanged. To see Wundt justly, therefore, we may best begin by going back more than half a century and examining the first of the six editions of the *Grundzüge*, the first rough draft of his permanent psychological system. Then we may go back still further to the earlier, discarded drafts, the *Beiträge zur Theorie der Sinneswahrnehmung*, 1858–1862, and the *Vorlesungen über die Menschen- und Thierseele*, 1863. Finally, we shall have to note how Wundt was influenced by contemporaries and predecessors.

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[1] E. B. Titchener, Wilhelm Wundt, this JOURNAL, 32, 1921, 176.

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The writing of the *Grundzüge* in 1874[2] was one of Wundt's two achievements which marked the launching of psychology as an autono-

[2]The first ten chapters were issued in 1873. On the changes in the plan of the *Grundzüge*, first outlined in a letter written by Wundt to his publisher, cf. E. B. Titchener, On the plan of the *Physiologische Psychologie*, this JOURNAL, 32, 1921, 596 f. The letter, a facsimile of which is inserted after p.90 in Wilhelm Engelmann, *Jubiläumskatalog*, 1911, reads, in translation, as follows:

Heidelberg, December 8, 1872.

My dear Sir,

For some time past I have been busy with putting into shape a book whose preparation has occupied me for several years and which I now design to publish under the title "Grundzüge der physiologischen Psychologie."

The field which in this book is to receive its first complete survey owes its demarcation and present importance almost wholly to investigations of the last two decades. It grew originally out of the physiology of sense-perception; and partly by way of the physiological investigations that bear upon that topic, partly by way of a series of independent psychophysical studies, it has gradually attained dimensions that make a summary survey of its whole extent and a development of the general scientific results to which these paths have led possible and, I believe, desirable. Although the new discipline has grown out of experimental physiology, the questions that interest it cannot in the nature of the case receive in the physiological texts the consideration that their importance demands—just as they lie also beyond the purview of current psychology. It therefore seems to me that physiological psychology is destined to form a middle term between physiology and psychology, in much the same way as at a somewhat earlier period physiological chemistry intervened between physiology and chemistry.

I have laid out the plan of my book as follows. I felt bound, first of all, to include in a physiological psychology the whole range of the interrelations between our outer life and our inner, psychological experience. But I have also felt that I could not avoid the question of the relation obtaining in the last resort between the worlds of inner and of outer occurrence, and of the explanation of the whole interconnection of psychological phenomena suggested by the survey of their borderland. I accordingly treat my subject in five sections. The first, introductory part deals with the physiological characters of the nervous system. The second contains the doctrine of sensation and apprehension; the third, the doctrine of organic movements. These two sections comprise the empirical material of physiological psychology proper; the one, in discussing sensation and apprehension, investigates the inner or psychological—the other, in discussing movement, investigates the outer or physiological consequence of the manifold interactions between our outer and inner experience. A fourth section then presents a criticism of psychological doctrines, directed and supported by the results obtained in these two; and a fifth and final section adds, as outcome of the whole enquiry, a general theory of psychophysical occurrence.

The complete book will fill some forty or fifty forms. Woodcuts are provided, especially in the first two sections, to illustrate the text. The work of preparation is so far advanced that the first steps toward publication can be taken in February of next year and the printing can proceed thereafter without interruption.

I now wish to ask, my dear Sir, whether, and if so upon what terms, you are disposed to undertake the publication of the book. Hoping to receive an early reply to this question, I am,

Yours faithfully,

W. Wundt

mous experimental discipline.[3] Affording us, as it does, the most comprehensive survey of the state of psychology in that momentous period, the book has a permanent value. We therefore begin our serial study of Wundt's psychology with a brief summary, chapter by chapter, of these first "Principles of Physiological Psychology."[4]

Introduction. While human life is the primary concern both of physiology and of psychology, the two sciences have so far, in dealing with their common subject-matter, been following different paths. Physiology has informed us of the phenomena of life as they may be perceived through our external senses; in psychology, man looked at himself, as it were, from within, and dwelled upon the results of this internal observation. Yet there is a wide range of vital processes that calls for coöperative exploring; sensation and centrally initiated movement are outstanding examples. A psychological fact, sensation is nevertheless referable in the main to conditions outside of the organism; and, while the various stages of sensory complication may be followed through psychological observation, such factors as order and grouping of impressions, interrelations of sensory and motor apparatus, and central processes of the nervous system need also to be considered. In the case of action, again, one must rely in large measure upon psychological observation in order to fill the gap between impression and movement.

So long, however, as physiology and psychology stand apart, the investigation of this common territory may be properly assigned to an intermediate discipline.[5] Such a border science would take over the problems which have, till fairly recently, been prosecuted exclusively by psychologists; and it would borrow its equipment from both parent-sciences, combining with psychological observation the methods of experimental physiology. It would single out actual instances of apprehension, feeling, and impulse for analysis; and it would analyze them, not directly, but through experimental variation of the relevant physiological factors. In time, it might provide the *Geisteswissenschaften*—history, law, politics, art, and religion—with a basis in natural science.

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[3]The other was the founding of his laboratory in 1879.

[4]Only a part (the physiological section) of the fifth edition of the book has been translated into English: *Principles of Physiological Psychology*, translated by E. B. Titchener, 1904. Cf. E. G. Boring, Edward Bradford Titchener, this JOURNAL, 38, 1927, 494.

[5]In his preface, Wundt writes: "The work which I here present to the public is an attempt to mark out a new domain of science. I am well aware that the question may be raised, whether the time is yet ripe for such an undertaking. . . . At the same time the best means of discovering the gaps in the subject-matter of a developing science is, as we all know, to take a general survey of its present status." The last sentence echoes a statement made ten years earlier by the Herbartian, M. W. Drobisch. Referring to Wundt's *Vorlesungen*, in a review entitled "Ueber den neuesten Versuch die Psychologie naturwissenschaftlich zu begründen" (*Zeitschrift für exacte Philosophie*, 1864, 313-348), Drobisch remarks: "He [Wundt] has preferred the freer form of lectures because of his conviction that text-books and systems abound in a science in proportion to lack of knowledge, and that psychology will never begin to progress unless an end is made to the writing of text-books and systems. On this score we should like to ask Mr. Wundt . . . . Is not a systematic arrangement best designed for calling attention to gaps within a science?" In his autobiography (*Erlebtes und Erkanntes*, 1920, 206), Wundt recalls Drobisch's scathing criticisms ruefully. Cf. *Vorlesungen*, I, ix.

A suitable name for this novel enterprise would be *physiological psychology*,[6] or *experimental psychology*.[7] Either designation would announce the advent of a natural, explanatory science, and would set it off from the current empirical psychology as well as from the newly launched *Völkerpsychologie*, both of which might well be termed descriptive sciences, or natural histories of mind.[8]

Sensation. Sensation is, unquestionably, the simplest psychological phenomenon; but it is never observed in isolation. To determine its underived properties, one must, in the manner of Condillac, first abstract from it all that it owes to its connections of the moment or to its history: relations in space and in time, feeling-tone, attribution to specific source. The remainder will consist of intensity and quality.[9]

Sensory intensity. Sensory intensity is commonly accepted as a simple indicator of external energies. To quote Herbart, two lights shine twice as brightly as one light, and three strings sound thrice as loud as one string. Yet it is quite obvious that such a relationship between sensory intensity and stimulus energy does not hold at the limits of sensitivity.

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[6]The term *physiological psychology* occurs as a title in F. W. Hagen, *Studien im Gebiete der physiologischen Psychologie*, 1847, and again in Leidesdorf and Meynert's *Vierteljahrsschrift für Psychiatrie in ihren Beziehungen zur Morphologie und Pathologie des Centralnervensystems, zur physiologischen Psychologie, Statistik, und gerichtlichen Medicin*, which began publication in 1867. Wundt himself employs the term in an article, *Neue Leistungen auf dem Gebiete der physiologischen Psychologie*, which he contributed to the first issue of the quarterly.

[7]Wundt seems to have coined the phrase *experimental psychology* in 1862 (*Beiträge*, vi). As we have not seen Bautain's *Psychologie expérimentale*, 1839, we cannot tell whether *expérimentale* in that title means experimental or empirical.

[8]We have not thought it worth while to analyze in detail the six chapters dealing with the "physiological properties of the nervous system," especially since a complete translation of the corresponding portion of the fifth edition is available.

The most important item in these chapters is the formulation of four "general laws of the central functions" (231): (1) the principle of connection of elementary parts: every nerve-element is connected with other nerve-elements, and only when so connected does it become capable of physiological function; (2) the principle of indifference of function: no element performs specific functions; the form of its function depends upon its connections and relations; (3) the principle of vicarious function: when the function of any element is inhibited or destroyed, it may be taken over by other elements, provided that they have the necessary connections; (4) the principle of localized function: every distinct function has a distinct place in the central organ from which it issues; *i.e.* a place in which the elements have the connections required for carrying out the function. The last three principles, we are told, are interrelated: vicarious function presupposes indifference, and qualifies localization. Wundt takes sharp issue, on the one hand, with Flourens' "principle of the indivisible function of the cerebral lobes" (226 and 234) and, on the other hand, with the "hypothesis of the specific function of the neural elementary parts" implied both by phrenology and by the Müllerian doctrine of the specific energy of nerves (226 and 232).

[9]Cf. H. Helmholtz, *Handbuch der physiologischen Optik*, 1867, 438: "I conclude that, in perception, whatever can be eliminated or reversed by causes of demonstrably empirical origin is not properly sensation, but the product of experience and habituation. It will be shown that, if we follow this rule, we must refer to sensation nothing but quality, and to experience and habituation most of the spatial perceptions."

Only the lower limits have been investigated in detail, and even there only such approximate values as the following are available. In an illumination 1/300 as bright as moonlight, we can just see a white paper strip at a distance of 5.5 meters; if a cork weighing 1 m grm. drops 1 mm., we can hear it from a distance of 91 mm.; we can feel a weight of 2 m grm.; we can sense a temperature 1/10°C. higher than that of the skin. The stimulus-threshold varies, moreover, with quality, place, and extent of stimulation: it goes up to 50 m grm. on certain parts of the skin; and it goes down for light and for temperature as area of stimulation is increased.

Between the lower and the upper intensive limits,[10] discrimination is measured in terms of the differential threshold, which varies somewhat with method of experimentation (just noticeable differences, average errors, right and wrong cases). All these methods, however, have yielded, at the hands of Weber and Fechner, the same generalization: sensory intensity is proportional to the logarithm of the intensity of stimulus. Thus it is that a shadow cast by moonlight disappears in lamplight, while a shadow cast by lamplight disappears in sunlight; for the same reason, stars are invisible in the daytime. On the other hand, slight differences in brightness between two clouds may be seen by the naked eye and through dark glasses with equal distinctness. The logarithmic relationship is also exemplified in the case of apparent stellar magnitude; and it has been experimentally confirmed in vision and in hearing, for pressure and for warmth (but not for cold), and for lifted weights.

The generalization of the facts of intensive discrimination that bears Weber's name is thus sufficiently assured. Exceptions to it, where any have been demonstrated, rest upon specific physiological causes. For Weber's Law itself, however, an immediate physiological ground is lacking. Observations made upon motor nerves, stimulated electrically at various stages of fatigue, suggest by inference that between stimulus-intensity and intensity of process in sensory nerve a simple proportionality prevails. Weber's logarithmic law may, consequently, be regarded as a psychological law, expressing the relationship of sensation to neural process. It will be shown later that the relationship holds for quality as well as for intensity.

Sensory quality. Each of the four special senses—hearing, sight, smell, and taste—provides a distinct continuum of highly differentiated qualities, although in taste and in smell the form of the continua is not apparent. The skin and all the remaining sensitive areas of the body may be regarded, with Johannes Müller, as one general sense-organ, the organ of the less differentiated sense of feeling.

Inter-modal differences are sufficiently accounted for by differences in the constitution of the sense-organs. Thus, the more primitive sense of feeling functions through undifferentiated and directly stimulated nerve-endings, some of which are rendered more sensitive by accessory bulbular (Krause) and corpuscular (Meissner, Pacini) structures. That temperature and touch rest upon different modes of stimulation of identical fibers is suggested by the confusion of the two at low intensities. The ear reacts first as a whole, but the cochlea with its basilar

[10]Wundt invents the term *Reizhöhe* for the terminal limen.

membrane superimposes an analytic mechanism. In the eye are the rods and the cones, every one of which is the seat of many complex chemical reactions. A lesser variety of chemical reaction-modes will allow for the discrimination—by way of smell and taste—of acids, bases, salts, ether oils, etc.

Such extraordinary functional specialization at the receptor removes any theoretic need for ascribing, with Müller and Helmholtz,[11] specific energies to peripheral or central nerves—an assumption that is, in any case, incompatible with evolutionary tenets, the principle of indifference of neural elements, and other established facts. Qualitative variety may, instead, be referred to heterogeneous molecular processes induced through receptor-stimulation and transmitted in their specificity to the brain. Repeated stimulation of the same sort will naturally predispose the adaptable nerve-substance to a set type of molecular movement. In hearing and in touch the molecular processes repeat the periodicity of the stimulus; in the case of sight, smell and taste, the relationship is not so simple. An account of auditory and visual quality will illustrate these two modes of receptor-function.

In general, auditory quality gives a faithful rendering of stimulus-form. The simple sine-wave is heard as a pure tone; the complex wave yields an analyzable sound; irregularity of wave-form is reflected in the noise. When one considers the continuity of tone and the unanalyzability of noise, the parallelism, it is true, seems to lose its precision; but these exceptions may be respectively attributed to the temporal limits of perception and to the "narrowness of consciousness." The parallel is further modified in the case of pitch-discrimination, where a logarithmic relationship obtains between sensation and stimulus. Here, moreover, the logarithmic function is not extracted from liminal determinations, but is plainly exhibited in the comparison of finite values.

The simple—or saturated—colors lie, like the simple tones, in a linear series. The color-line, however, returns on itself and—with the purples—forms a closed figure, which becomes a triangle when degrees of color-saturation are added. Since we regard color-quality with its neural correlate as a continuous but complex function of stimulus, the color-triangle may be taken as a graphic representation of that function. Color-differences are measured in liminal terms; and the colors coinciding with maxima and minima of differential sensitivity—red, yellow, green, and blue—have a more pronounced quality than the other colors, which are sensed as transitions.

Tonal quality, being independent of intensity, is not influenced by fatigue; in the case of color, however, where quality changes with intensity, fatigue affects both. Color passes over into gray or black as the stimulus is continued; and, unless contrast intervenes, the complementary after-image—itself an indication of altered retinal sensitivity—is negative (less bright than the original impression). The same-colored after-image is always positive (as bright as, or brighter than,

[11]Wundt had earlier followed Helmholtz's views concerning specific energy (cf. *Grundzüge*, 346 f. with *Vorlesungen*, I, 182), theory of vision (cf. *Grundzüge*, 332 with *Vorlesungen*, I, 158), and theory of hearing (cf. *Grundzüge*, 348 with *Vorlesungen*, I, 184).

Upon these issues, and others to be noted later, Wundt remained in agreement with Helmholtz down to the third edition of his *Lehrbuch der Physiologie*, which he published the same year (1873) in which he sent the *Grundzüge* to press. Cf. *Lehrbuch*, 494, 611, 671 f.

the original impression) and implies lag of excitation; while the flight of colors may be explained by differences in the temporal course of retinal excitation.

The facts of color-blindness are compatible with the hypothesis that, in vision, receptor-activity is a continuous function of wave-length. Color-blindness means, as a rule, low sensitivity or total insensitivity for red, combined with low sensitivity for violet. Insensitivity for red occurs also in the normal retinal periphery, in the initial stage of atrophy of the optic nerve, and in many cases of simple amblyopia. The occurrence of green-blindness, blue-blindness, and achromatopsy lacks proof.

The most favorable conditions for contrast, as demonstrated in the Meyer and Ragoni-Scina experiments, are: maximal difference of color-tone; an optimal brightness-difference (white on black is no brighter than light-gray on the same black);[12] an optimal saturation of inducing-surface; zero saturation of induced surface; and absence of contours (in Fechner's experiment the contrast-shadow persists when viewed through a tube). While, then, the last factor displays the influence of reproduction, favored by the perception of discrete objects, the others suggest that discrimination is relative in the case of visual quality, as it is in the case of intensity and auditory quality. Direct proof of this relativity may be found in the fact that brightness-contrast remains undisturbed under wide changes of illumination; and no facts are contradicted by generalizing from this special case to the whole group of contrast-phenomena. To dispose of the phenomena of contrast as illusions of judgment (Helmholtz)[13] is to ignore their palpable character and to exalt the secondary factor of reproduction; while to refer them to a supposititious interaction of retinal areas is to disregard the reproduction-factor altogether. Rather do the facts of sensation, taken together, require the positing of a general psychological law of relation, valid for quality as well as for intensity. Sensation, in both of its aspects, is a relative, and not an absolute measure of neural process.

Sense-feelings. Sheer pleasantness and unpleasantness depend, in the first instance, upon degree of sensory intensity. Hence, these feeling-tones are most noticeable in the lower senses, where qualitative distinctions are few; and there they contribute to the apprehension of self and to mood and temperament. In the higher senses, qualitative variety is matched by a corresponding diversity of feelings, which enter into the esthetic appreciation of external objects. High tones are gleeful, low tones have dignity; loud tones give the impression of power and exaltation, soft tones are low-spirited; there is rejoicing in a fast, moodiness in a slow tempo; and clashing, discordant feelings arise from the interplay of fundamental with overtones and the intrusion of beats. Similarly, cheer is found in white, gravity in black; excitement in yellow, depression in blue; and between these poles stand the neutral grays, the tranquil greens, and the instable reds. Affective analogies—such as obtain between deep tones and dark colors, and are

[12] Cf. 408.

[13] Wundt writes, "I then [*Vorlesungen*, I, 198; *Lehrbuch*, 626], adhered in detail to Helmholtz's theory of contrast; but the facts have convinced me of its untenability (*Grundzüge* 423)."

Wundt also recedes from his previous concurrence with Helmholtz in the explanation of color-mixture (cf. *Grundzüge*, 387 with *Lehrbuch*, 609 f.), of after-images (cf. *Grundzüge*, 398 f. with *Lehrbuch*, 620, 624) and of the flight of colors (cf. *Grundzüge*, 401 with *Lehrbuch*, 622 f.).

implied in terms like 'color-tone,' 'tone-color,' 'cold and warm colors,' 'sharp tones,' and 'saturated colors'—contribute further to esthetic appreciation.

While feeling is thus originally determined by intensity and quality, it may be modified through associations. The feeling-tone of black, for example, is enhanced by its association with mourning; but the association, in turn, rests upon the original feeling-tone of black. Other affective connections, such as between green and the peaceful meadow, or between yellow and the cheering sunlight—instances of a remarkable harmony between sensation and external nature—illustrate the principle of adaptation to environment which has governed the development of the human eye.

The inconstancy of feeling is explained, in part, by sensory adaptation and summation. Yet such instances as the euphoria of convalescence and the thrill found in games of chance, in dramatic performances, and in other forms of play suggest that it is of the nature of feeling to rebound from one of its poles to the opposite pole. As Kant said, pleasure must be preceded by pain. The basis for this affective movement, and in fact for feeling itself, is to be found in the relation of sensation to consciousness, which continually oscillates between the opposed states of inhibition and facilitation. Pleasantness goes with facilitation, unpleasantness with inhibition; witness the assertiveness of pain, the easy flow of ideas when one is pleased, the frustration of sight by black, the clash of sensation in beats.

The psychological status of feeling is now apparent. Every sensation has its feeling-tone just as it has its quality and its degree of intensity. But the three sensory determinations do not stand on a level; feeling is dependent upon intensity and quality, and represents a secondary modification imposed upon sensation by the general state of consciousness.[14]

Apprehension. In the apprehension of existing and non-existing objects, of external objects and of oneself, sensations—supplied by peripheral or central stimulation—are involved. But these sensations are now ordered both in space and in time. Apprehension, therefore, rests upon a new, integrative activity. The two modes of integration, temporal and spatial, are found together, and in their earliest form, in the tactual perception of bodily movement; they are independently and more highly developed in hearing and in vision.

Tactual perceptions. Tactual localization without the aid of vision implies, in the first place, a local coloring of cutaneous sensations, or local signs, whereby parts of the body are discriminated. Such local differences would account for the limits of localization (expressed in terms of constant error or two-point limen, and pictured by means of overlapping 'sensory circles') as well as for the effects upon localization of practice, sensitivity, and intensity of stimulus. The correlation of size of sensory circle with nerve-supply of the skin points to a structural basis for local signature.

Before, however, the bidimensional system of qualitative local signs can be converted into a spatial continuum, some precise and dependable metric means is required; and only in the fine intensive grading of the sensations of motor innervation is a device of this sort discoverable. Localization may, then, be referred to a

[14]The theory of feeling is given a different turn later in the chapter on Consciousness and Attention.

synthesis of motor and tactual sensations resting ultimately upon a coördination of tactual and motor impulses, which presumably takes place in the optic thalmus but is also specifically represented in the cortex. In this psychological synthesis,[15] as in a chemical synthesis, the constituents—motor sensations and local signs—lose their identity in the product, spatial order; they cannot, therefore, be observed for themselves, but must be inferred from the conditions—motor and cutaneous—of localization.

The early development of the spatial discrimination of parts of the body must have been favored by the unambiguous relation between motor and tactual sensations in bodily movement. The tactual exploration of objects would then lead to the perception of surfaces of varying curvature; and, because of the predominance of rectilinear movement, the object-surfaces would be fitted into a tridimensional framework. At the same time, the possibility of reproduction would render unnecessary a constant reliance upon both local signs and movement. Since bodily movement, with its regularly recurring accents or emphases, also provides a basis for temporal apprehension, it would seem that the earliest perception of all was that of movement, a perception in which temporal and spatial order—later developing separately in hearing and in vision—are given together.

Auditory perceptions. Sounds owe their localization altogether to vision and to touch; specific factors are to be looked for in tensor tympani, pinna, and throat movement. The variety of sounds, their analyzability, and their precise attunement to stimulus-course come into account in other ways: in the apprehension of rhythm and time, of melody, and of tonal relationship.

Related sounds are those containing common or coincident partials. Where coincidence is independent of the fundamental, as in the vocal sound and in timbre, relationship is constant. In the musical interval, on the other hand, there is a variable—or harmonic[16] —relationship between the constituent notes, the degree of resemblance depending upon the relative number and intensity of the common partials. The octave, double octave, and twelfth represent imperfect unisons rather than related tones, for in each case the higher note merely repeats partials already present in the lower note. The closest degree of kinship, then, is to be found in the fifth, with its three audible common partials; next come the fourth, the major sixth, and the major third, with two such partials; and lastly the minor third and other intervals, with but one. In major chords, where the difference-tone furnishes a common fundamental, relationship is indirect. To such an indirect relationship pure tone-intervals owe their harmonic character, although associations may be contributing factors.

Suitable conditions for temporal perception and the perception of rhythm are first found in connection with bodily movement, and later in connection with the visual perception of movement.[17] Hearing, however, affords the most adequate

[15]How Wundt came upon the notion of synthesis, and the changes which it went through between 1858 and 1874, are related by E. B. Titchener, A note on Wundt's doctrine of creative synthesis, this JOURNAL, 33, 1922, 351 ff. Cf. also *Lehrbuch*, 554 f.

[16]Earlier Wundt followed Helmholtz's theory of harmony (cf. *Grundzüge*, 370 f. with *Vorlesungen*, II, 68).

[17]Wundt alters his account of the perception of time in a later chapter (cf. *Grundzüge*, 513 and 681 f.).

basis for both perceptions. Temporal discrimination is present in the simplest rhythm; every intensive rise is apprehended as a repetition, and with every rise or fall a recurrence is anticipated. Duration is estimated in terms of the rhythmic unit, and is correctly estimated only within the limits of rhythmic organization.

In melodic apprehension, qualitative variation is added to the intensive variation upon which rhythm is conditioned; hence the significance of variable tonal relationship (melodic repetition and the law of the tonic) for melody, and of constant tonal relationship (rhyme and assonance) for poetry.

Visual perceptions. The retinal mosaic is fundamental to visual localization. It provides the sensory material for spatial arrangement; and it determines the limits of the total field of vision, and of local discrimination, or visual acuity. Since the cones have more numerous fiber-connections and are more closely packed than the rods, acuity falls rapidly from fovea to periphery. Discrimination varies also with form and size of stimulus-object; and peripheral acuity in particular improves with practice.

The determining factor in visual localization, however, is ocular movement. The orientation of the eye and the direction and magnitude of its excursions are apprehended by way of sensations of innervation and tactual local signs. That these motor and cutaneous sensations enter into fairly uniform connections with retinal local signs is guaranteed by the laws of Listing and Donders; and when the eye is stationary, the impulses of innervation reproduce the appropriate tactual local signs. From the synthesis of these elements, as from the similar synthesis in touch, spatial arrangement results in the form of a surface. The distance of this surface from the eye is defined, rather poorly, by way of accommodation; more precisely, by way of convergence; and, at a later stage of development, by the reproductive effect of perspective. Specifically, the influence of eye-movement evidences itself in the orientation, form, and continuity of the visual field; in the displacement of objects owing to paralysis of eye-muscle; in the discrimination of extents; in the overestimation of vertical distances (except in the case of such familiar figures as the circle) and of distances in the upper and outer portions of the field of vision; in the apparent inclination of verticals; in the overestimation of filled extents; and in the various illusions of visual movement.

This genetic theory of visual perception presupposes sensory and motor mechanisms that are ready to function—for purposes of spatial apprehension—at birth (or, in the case of congenital blindness, as soon as vision is rendered possible); it does not derive perception from empirical inferences or from associations. Perception, which is the basis of experience, cannot itself be the result of experience; and the concept of association is quite different from that of sensory synthesis, since an association does not yield a new product.

The singleness of binocular vision rests, not upon stimulation of identical points, but, in the first instance, upon a common localization made possible by the synergy of binocular movement. Through repeated movements of fixation, a paired correspondence of retinal points becomes established which favors, but does not compel, single vision. Common localization is facilitated also by eye-movement, by the presence of fixation lines (if not too long) and of secondary cues to localization, by familiarity of object, and by similarity of the binocular impressions.

The visual field is properly a plane: one cannot see two objects in the same visual direction, at least not with equal distinctness. Of two sufficiently different impressions, one impression prevails altogether (perfect mirroring, retinal rivalry and suppression), or one is more distinctly perceived than the other impression (transparency and mirroring); if the factors favoring each impression balance, luster appears.

Imaginative apprehension. The sensory materials involved in perception recur, in conformity with the rules of association, either as direct supplements to perception or as discrete formations. Thus arise various forms of apprehension: memory and imagination, hallucination, illusion, and dream—all implying a spread of excitation in the sensory areas of the cortex. Normally this secondary excitation is weak, and the reproduced elements are faint; hence, for example, pleasant incidents lose less of their body in recall than painful (and therefore more intensive) experiences do. Cortical excitation may spread further to motor areas; and, in the case of hallucinatory objects that follow the movement of the eyes and leave negative after-images, a centrifugal course of excitation is indicated. In the hallucinatory types of apprehension, moreover, cortical sensitivity is high, and for various reasons: absence of stimulation (silence is conducive to auditory, darkness to visual hallucinations); excessive stimulation (hence the frequency of visual hallucinations); or nutritive disturbances leading to an accumulation of products of decomposition in the cortex. To this last condition, a source of automatic central stimulation, dreams and pathological hallucinations owe their incongruency; yet they draw largely upon reproductions instigated by sensory stimulation and affective tone.

Complex apprehension, generalized apprehension, and perceptive forms. Apprehension is enriched through the concurrence and reproduction of impressions of disparate modalities. This is exemplified in perceiving a sharp point, a grating noise, a heavy weight, an impending blow. Movement also contributes to such complex apprehensions by way of the sensory materials involved in gesture, speech, and writing. Pantomime, onomatopoea, and pictography exemplify the complication or fusion of similar elements, while the association of sounds with visual and other disparate impressions was originally guided by analogies of feeling. For the most part, however, it is sheer habituation that has produced the intimate connection of object, word, and sign.

The change from a specific to a generalized or schematic apprehension of objects requires, not a distinct process of abstraction, but the normal operation of the laws of association. Generalization begins in perception: the features that recur in objects of the same class tend to stand out, while the variable features are neglected. In this way a schematic mode of apprehension develops, which, however, is not stable, but easily resolves itself into a serial apprehension of specific objects within a class. Even animals may attain to this level of generalization; and the child who identifies a circle first as a plate, then as a pond, and then again as the moon, is carrying out the process of resolution, and thereby beginning to comprehend a class of objects.

Schematization, however, is adequate only for certain classes of objects, such as 'circle' or 'green.' For most object-classes the schema becomes too vague, and

the process of resolution too cumbersome (*e.g.* 'color'). The realization of this difficulty, at first felt but vaguely, finally leads to the postulation of a generalized object. Such a postulate, or concept, implies no new psychological mode other than schematic apprehension. With the advent of the concept the verbal element in apprehension becomes more prominent; and the verbal symbol is then also pressed into service for the comprehending of relations (such as means and end), to which schematic apprehension is altogether inadequate. The abstract concept —the theoretical as a postulate of thinking, the practical as a postulate of action— develops out of an earlier stage of exemplificatory apprehension (*e.g.* idol-worship precedes the abstract conception of god). To this derivation abstract concepts still owe their feeling-tone and their appearance in correlative pairs.

When thinking of space or time—the two forms of perception—we apprehend a specific stretch of space or of time, and not a symbol as with other concepts. Yet their mode of derivation is similar. The geometric representation of the systems of color and tone neatly illustrate the formation of the space-concept; by bracketing together color-tone, saturation, intensity, and duration, we may even understand a space of four dimensions. It is, nevertheless, inadmissible to suppose, as Zöllner does, that space is curved; for while physical theory may—as it has already done— advance far beyond the direct evidence of perception, it must conform to the general perceptive forms of space and time. On the other hand, psychology has nothing to say concerning the reality of space or of any other concept, or even of the objects of perception; it takes all these for granted, and then seeks to determine the conditions of their apprehension.

Esthetic feelings. At a level between sense-feelings and emotions, one may place the varied class of feelings that color all apprehension, and whose basal modes are liking and disliking. Often indistinct, these 'esthetic' feelings are most obvious in tonal harmony and rhythm, and in the symmetry and proportion of spatial form. But they are also to be discerned in recall and in recognition, in the simple understanding of a sentence and in the solution of a problem, in the ideas of the beautiful and the ugly, the sublime, the ridiculous and the comic. It is evident that these feelings imply evaluation. They may be said to reflect the degree to which the diversity given in apprehension is unified. They thus express the general conscious status of apprehension, just as the sensory feelings indicate the status of sensation.

Consciousness and attention. Conscious life does not begin with sensation; nor does it involve from the outset a consciousness of self; nor, finally, can it spring from unconscious inferences.[18]

Self-awareness and inference, both, appear at a fairly late period of development; they imply the use of concepts. The explicit awareness of self, moreover, presupposes, first, the perceptive discrimination of external objects from one's own body and, secondly, the differentiation of memory and imagination from perception.

[18]In the last two clauses, Wundt negatives the fundamental ideas of the *Beiträge* and the *Vorlesungen*. Even sensation had been made out to be an unconscious inference (*Beiträge*, 445 ff.; *Vorlesungen*, I, 52 ff., 57 ff., 305.).

The single sensation, on the other hand, is an abstraction. The sound we hear has a certain duration; the color we see is localized. Synthesis operates from the beginning; and wherever synthesis, even of the most rudimentary kind, is rendered possible by conditions of central organization, some level of consciousness must therefore be assumed to exist. In animals possessing various levels of neural integration, the unitariness of consciousness is nevertheless guaranteed by the close interrelation of the various neural mechanisms. Some animals, it is true, exhibit signs of consciousness after decerebration; such consciousness, however, is not residual, but a new development.

Alongside of sensory synthesis and of association, a third psychological activity must now be recognized: attention or apperception. All objects are not apprehended on equal terms. Under instantaneous illumination, several words can be clearly read; if, however, one is intent upon the precise form of a single letter, all the other letters become less clear; while with repetition the range of the attentive 'point of regard' may increase. The same phenomena can be demonstrated with a combination of tones. The wider range of the whole conscious field of regard may be inferred from the limits of rhythmic complexity and of consecutive thinking.

While attention is favored by reproduction (as the aforementionede ffect of repetition indicates), and also by stimulus-intensity and visual fixation and contours, various facts bespeak for it a single primary subjective condition. Under attention, apprehension of disparate objects (for example, the sound of the bell and the sight of the pointer in the complication experiment) is unitary. Then there is the peculiar feeling that may be observed during the accommodation of attention. It bears a personal reference; it becomes fairly tense in expectation and surprise, and in effortful thought, imagination, and recollection; it may be accompanied by innervation of the muscles about the eyes, ears and head; it suggests, finally, innervational functioning. One may, therefore, conceive of the cortical innervation-areas as operating in two ways: they shunt the course of excitation back to the sensory spheres, whose activity is thus reënforced; and they also innervate the bodily musculature. It follows that between attention and voluntary action there is simply a difference in the relative emphasis of these two innervational modes. The central determination of all feelings, which we have earlier derived from the total state of consciousness, may now, also, be more specifically referred to innervation.

The course of apprehension; association. Thec ourse of apprehension is regulated by the accommodation of attention. The shortest 'physiological time'[19] is obtained under conditions permitting adequate accommodation: foreknowledge of the object reacted to and of the time of its appearance, and absence of distraction. Accommodation also governs the time-relations both of perceived objects (cf. the 'personal equation')[20] and of memorial objects (cf. the experiments on the comparison of time-intervals).

[19]Wundt here (*Grundzüge*, 733) reports experiments performed by himself and his wife.

[20]Wundt brought the 'personal equation' into the laboratory in 1860, when he devised the complication experiment. In 1862, in the course of a general attack on Herbartianism, he made the bold announcement that this experiment disproved the metaphysical axiom upon which the whole Herbartian psychology had been

The possibility of reproduction rests upon a facilitating after-effect of cortical excitation. The after-effect is of a functional sort, analogous to the effect of practice upon muscular movement or upon visual acuity rather than to the after-effect of visual sensation. Such a dispositional after-effect underlies the laws of association by spatial and temporal contiguity. These two laws, both of which imply a tendency of excitation to recur in the manner to which it has been habituated, may be combined into a single law of associative habituation. The law of similarity presupposes, in addition, a transfer-effect of associative practice. Since the sensory areas of the cortex are dominated by the frontal areas of innervation, the laws of association cannot operate automatically. They are always subject to the accommodation of attention, although this may sometimes, as in dreams and in the pathological flight of ideas, be fairly slack; and the guiding influence of accommodation grows stronger with proper training. What appears to be association by contrast really points to a peculiarity of attention: when accommodation continues for a long time in the same direction, fatigue sets in, encouraging a maximally different direction of accommodation.

Emotion, impulse, and temperament. Emotion and impulse are both expressions of innervational activity. In emotion, attention is, as it were, taken by surprise; in impulse, it is in a state of expectation.

In all pronounced instances of emotion, the affective situation inhibits the flow of ideas; if the emotion is pleasant, a sudden release of inhibition may follow; all emotions gradually subside, and are replaced by moods. At the same time, the changing state of innervation and of feeling is reflected in sthenic and asthenic tonicity and in alteration of visceral function; this bodily reverberation is the same in all emotions of sufficient intensity.

The impulse is characterized by a forward reference to an affective situation, with appropriate motor consequences. Its two opposed modes of desire and resistance meet in the neutral mode of expectation. The mere accommodation of attention may thus be regarded as an elementary impulse. The more primitive impulses, like the early perceptions, command ready-made sensory and motor mechanisms; yet they are not mere reflexes. Their direction, at first vague, is defined only as apprehension develops. Nor does psychological development con-

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erected. Herbart, by the use of his mathematical method, had demonstrated that the sum of ideas held simultaneously in consciousness was never less than two, while the relative displacement of bell and pointer in the complication-experiment —so ran Wundt's argument—meant that we can apprehend only one thing at a time (*die Einheit der Vorstellung*). Cf. *Beiträge*, xxi (footnote), xxvii f., 335, 382 f.; *Vorlesungen*, I, Lectures III and XXIII, and 469; *Grundzüge*, v, 757; J. F. Herbart, *Psychologie als Wissenschaft*, sec. 44; E. B. Titchener, Wundt's address at Speyer, 1861, this JOURNAL, 34, 1923, 311; E. G. Boring, *A History of Experimental Psychology*, 1929, 247 ff., 341. Drobisch replied vehemently for the Herbartians in the review mentioned above. In the *Grundzüge*, Wundt tacitly drops the argument.

Boring writes (*op. cit.*, 144) that "Müller's belief in the instantaneousness of nerve action was overthrown by Helmholtz's measurement of the velocity of the nervous impulse, but his view nevertheless persisted as the basis of Wundt's psychology of the 'complication.'" Cf., however, *Vorlesungen*, I, Lecture III, where Wundt writes "Just a few years ago, a German physiologist, Helmholtz, determined with precision the true rate of conduction of sensory and motor process in nerves" (33), and then uses Helmholtz's determination to support his own explanation of the complication experiment.

sist in the elimination or repression of the primitive impulses, but in the appearance of new and more varied forms, which, of course, bring with them the possibility of conflict.

Emotional and impulsive dispositions, or temperaments, vary more or less with individual, nation, race, and species. One may discriminate, with Galen, four temperamental types. Intense emotion and dark mood distinguish the choleric and the melancholic from the phlegmatic and the sanguine. While the sanguine and the choleric change their mood with the occasion, the brooding and the conservative types are not so easily distracted. Lastly, one may set off the affectivity of the melancholic and the sanguine against the activity of the choleric and the phlegmatic. Each temperament has its good and its bad points; the wise man will, therefore, cultivate all of them.

Action. There is no sharp cleavage between voluntary action, or action initiated by impulse, and non-voluntary action. The impulse usually indicates the general direction of the action, without prescribing its specific course. Under repetition, intention may lapse altogether or may show itself only in starting the actional sequence and in bringing it to a close. Such facts cannot be accounted for by invoking now the agency of the will and now unconscious or mechanical determination. All actions are sustained by the operation of organic devices. In voluntary action, essentially the same mechanisms are involved as in non-voluntary action; but initiation is cortical, since the impulse implies innervational activity. The course of apprehension resembles that of action: once started by apperceptive activity, it may be carried on by way of associative linkage with only occasional interference on the part of apperception. The two modes of innervational function thus behave in a like manner.

Expressive movements. Expressive movements have various origins. The tonic and visceral indices of emotion are, for the most part, direct consequences of innervation; expressions of the mouth and nose are specific sensory reactions that have become integrated with diverse situations by virtue of a common affective coloring; and gestures can be traced to early perceptive situations. Speech began as vocal gesture; as such it may still be observed in the deaf-mute. Both speech and gesture presuppose a discreteness of perceived objects that is lacking in animals and in human infants. The further development of speech was aided by onomatopoea and affective analogy. The comprehension of speech was facilitated by gestural comment, just as facial expression contributed to the understanding of gesture.

Concluding remarks. All psychological processes—sensation, synthesis, association and apperception—are physiologically grounded; and just as the organism is a unity of many parts, so mind, its inner counterpart, is a unitary connection of multiple processes. These principles suggest the monistic conception that the world consists of simple beings variously integrated, and undergoing parallel changes externally and internally. When integration becomes sufficiently persistent and coherent—a level attained in animal life—the internal processes take the form of sensation and apprehension. In this sense, mind may be regarded, with Leibnitz, as a mirror of the world.

WUNDT'S DOCTORATE STUDENTS AND THEIR THESES
1875–1920

By MILES A. TINKER, University of Minnesota

As an historical figure, Wundt is rapidly achieving the eminent position earned by his contributions to the advancement of scientific psychology. The centennial of the birth of this "senior psychologist in the history of psychology" was August 16, 1932. A survey and list of the theses done under his guidance during his professorship at Leipzig may be fittingly given at this time.

Since 1885 Leipzig theses and their authors are listed in the successive volumes of *Jahresverzeichnis der an den deutschen Universitäten erschienenen Schriften*. There is no published list prior to that time. The titles and names listed below were obtained for the period ending 1884 from the records filed at the University of Leipzig and since 1884 from the *Jahresverzeichnis*.

From 1875 to 1919 Wundt directed 186 theses. The frequencies by quinquennia follow: 1875–80 (6 years), 12 degrees; 1881–85, 23 degrees; 1886–90, 26 degrees; 1891–95, 28 degrees; 1896–1900, 23 degrees; 1901–05, 25 degrees; 1906–10, 25 degrees; 1911–15, 22 degrees; 1916–20, 2 degrees. While during the first six-year period less than two degrees per year were granted by Wundt, from 1880 to 1915 he sponsored about five per year and they were spread quite uniformly through the seven quinquennia. This is an impressive record, especially impressive when one remembers that Wundt was 83 years old in 1915. Although he retired in 1917 Wundt was major adviser on two theses completed in 1919. The fact that Wirth was minor adviser on several of the theses granted by Wundt between 1911 and 1920 indicates that the latter was probably transferring some of his responsibilities to his colleagues during this period. The first Leipzig thesis sponsored by Wirth as major adviser is dated April, 1919 (1921) in the 1921 *Jahresverzeichnis*.

In considering the Wundtian doctorates the reader should remember that many students (not listed here), who did not take their degrees at Leipzig, studied in Wundt's laboratory. Wundt was, of

course, minor adviser on numerous theses (not tabulated here) in philosophy, pedagogy, ethics, and anthropology. The list of the 186 men obtaining degrees and their thesis titles follows:

1876
STEFFEN, ROBERT. Kants Lehre vom Dinge an sich.
SWIENTOCHOWSKI, ALEXANDER. Ein Versuch die Entstehung der Moralgesetze zu erklären, eine ethische Analyse.

1877
DIMITRESCO, CONSTANTIN. Der Schönheitsbegriff, eine ästhetischpsychologische Studie.

1878
BOLLIGER, ADOLF. Das Problem der Causalität.
FISCHER, WILHELM. Herder's Erkenntnislehre und Metaphysik.
HARTUNG, ERNST BRUNO. Grundlinien einer Ethik bei Giordano Bruno, besonders nach dessen Schrift Lo spaccio de la bestia trionfante.
SOBCYC, PETER. Das Pythagoreische System in seinen Grundgedanken.

1879
GUDE, WILHELM. Die Gesetze der Physiologie und Psychologie über Entstehung der Bewegungen und der Articulationsunterricht der Taubstummen.
TZERTELEFF, DIMITRIJ. Schopenhauer's Erkenntnistheorie: Eine kritische Darstellung.
VUJIC (WUITSCH), MICHAEL. Über Substanz und Causalität.

1880
RADESTOCK, PAUL. Schlaf und Traum. Eine physiologisch-psychologische Untersuchung.
TISCHLER, OTTO. Ostpreussische Gräberfelder.

1881
BORRIES, GOTTFRIED. Über den Pessimismus als Durchgangspunkt zu universaler Weltanschauung.
DONADT, ALFRED. Das mathematische Raumproblem und die geometrischen Axiome.
FRIEDRICH, MAX. Über die Apperceptionsdauer bei einfachen und zusammengesetzten Vorstellungen.
HORNY, FRIEDRICH WILHELM. Kurze kritische Darstellung der Anfänge und Entwickelung der utilitarischen Moralphilosophie in England.
KÖNIG, EDMUND. Über den Substanzbegriff bei Locke und Hume.

1882
SCHMERLER, G. B. TH. Untersuchungen über den Farbencontrast vermittelst rotirender Scheiben.
TISCHER, ERNST TH. Über die Unterscheidung von Schallstärken.
TRAUTSCHOLDT, MARTIN. Experimentelle Untersuchungen über die Association der Vorstellungen.

1883
ESTEL, VOLKMAR. Neue Versuche über den Zeitsinn.
KORNER, CHRISTIAN REINHOLD. Die logischen Grundlagen der Systematik der Organismen.
MERKEL, JULIUS. Die zeitlichen Verhältnisse der Willenstätigkeit.
SCHNEIDER, GEORG. Der menschliche Wille vom Standpunkte der neueren Entwickelungstheorien.

1884
ROSENSTEIN, ALFRED. Die psychologischen Bedingungen des Bedeutungswechsels der Wörter.
STERN, ALBERT. Über die Beziehungen Chr. Garve's zu Kant, nebst mehreren bisher ungedruckten Briefen Kant's, Feder's und Garve's.

1885

BERGER, OSCAR. Über den Einfluss der Reizstärke auf die Dauer einfacher psychischer Vorgänge mit besonderer Rücksicht auf Lichtreize.

BIXBY, JAMES THOMPSON. Herbert Spencer's Data of Ethics and Transmutational Ethics.

DIETZE, GEORG W. Untersuchungen über den Umfang des Bewusstseins bei regelmässig aufeinanderfolgenden Schalleindrücken.

KNABE, CARL. Die Formen des indirecten Beweises mit besonderer Rücksicht auf ihre Anwendung in der Mathematik.

LORENZ, GUSTAV. Die Methode der richtigen und falschen Fälle in ihrer Anwendung bei Schallempfindungen.

MEHNER, MAX. Die Lehre vom Zeitsinn.

MÜNSTERBERG, HUGO. Die Lehre von der natürlichen Anpassung in ihrer Entwickelung, Anwendung und Bedeutung.

NEDICH, LJUBOMIR. Die Lehre von der Quantification des Prädicats in der neueren englischen Logik.

SELVER, DAVID. Der Entwicklungsgang der Leibniz'schen Monadenlehre bis 1695.

1886

CATTELL, JAMES MCKEEN. Psychometrische Untersuchungen.

GÜHNE, HERM. BERNH. Über Hobbes naturwissenschaftliche Ansichten und ihren Zusammenhang mit seiner Zeit.

KÖHLER, ALFRED. Über die hauptsächlichsten Versuche einer mathematischen Formulirung des psychophysischen Gesetzes von Weber.

LANGE, LUDWIG. Der Bewegungsbegriff während der Reformation der Himmelskunde von Copernicus bis zu Newton (1543 bis 1687).

STARKE, PAUL. Die Messung von Schallstärken.

WOLFE, HARRY KIRKE. Untersuchungen über das Tongedächtniss.

1887

EPSTEIN, JOSEPH. Die logischen Principien der Zeitmessung.

GLASS, RICHARD. Kritisches und Experimentelles über den Zeitsinn.

KÜLPE, Oswald. Zur Theorie der sinnlichen Gefühle.

SCHANZ, JOHANNES. Das Freiheitsproblem bei Kant und bei Schopenhauer, eine kritische Untersuchung.

1888

BOBTSCHEFF, (W.), NICOLAUS. Die Gefühlslehre in ihren hauptsächlichsten Gestaltungen von Kant bis auf unsere Zeit.

GLOECKNER, GOTTFRIED. Der Gottesbegriff bei Leibniz.

LIPPS, GOTTLOB FRIEDRICH. Die logischen Grundlagen des mathematischen Functionsbegriffs.

LUFT, EDUARD. Über die Unterschiedsempfindlichkeit für Tonhöhen.

THIEME, KARL. Der Primat der praktischen Vernunft bei Lotze.

1889

BRIX, WALTER. Der mathematische Zahlbegriff und seine Entwicklungsform.

SCHISCHMANOW, IWAN. Untersuchungen über die Empfindlichkeit des Intervallsinnes.

ZEITSCHEL, HERMANN RICHARD. Die Erkenntnislehre Spinoza's.

1890

FISCHER, LUDWIG. Cogito ergo sum.

GIMKIEWICZ, BENNO. Über Herbarts Methods der Beziehungen.

KIRSCHMANN, AUGUST. Über die quantitativen Verhältnisse des simultanen Helligkeits- und Farbencontrastes.

KRESTOFF, KRESTO. Lotzes metaphysischer Seelenbegriff.

LORENZ, CARL. Untersuchungen über die Auffassung von Tondistanzen.

SCHMID, ALBERT. Zu Kants Lehre vom Raum.

SCHUBERT, JOHANNES. Adam Smith's Moralphilosophie.

TÜRK, HERMANN. Das psychologische Problem in der Hamlet-Tragödie.

1891
ANGELL, FRANK. Untersuchungen über die Schätzung von Schallintensitäten.
BENECKE, MAX. Vom Takt in Tanz, Gesang und Dichtung.
BONDI, GEORG. Das Verhältnis von Hallers philosophischen Gedichten zur Philosophie.
PACE, EDUARD. Das Relativitätsprincip in Spencer's psychologischer Entwickelung.
SCHLEIMER, ALEXIS. Der Positivismus, eine kritische Studie.
SCRIPTURE, EDWARD. Über den associativen Verlauf der Vorstellungen.

1892
REES, DANIEL. Contemporary English Ethics.
TITCHENER, EDWARD. Über binoculare Wirkungen monocularer Reize.
UHLITZSCH, RICHARD. Anthropometrische Messungen und deren praktischer Werth.

1893
ECKENER, HUGO. Untersuchungen über die Schwankungen der Auffassung minimaler Sinnesreize.
GRUBER, EDUARD. Experimentelle Untersuchungen über die Helligkeit der Farben.
KÄMPFE, BRUNO. Beiträge zur experimentellen Prüfung der Methode der richtigen und falschen Fälle.
PAULINS, JOHANNES MARTIN. Die Sittenlehre Geulincx.
RADULESCU-MOTRU, CONSTANTIN. Zur Entwicklung von Kant's Theorie der Naturcausalität.
RICHTER, RAOUL. Schopenhauers Verhältnis zu Kant in seinen Grundzügen. I. Theil.
STIER, THEODOR. Analyze und Kritik der Berkeley'schen Erkenntnistheorie und Metaphysik.
WENZEL, ALFRED. Beiträge zur Logik der Sozialwirtschaftslehre.
WEYGANDT, WILHELM. Entstehung der Träume.
WITMER, LIGHTNER. Zur experimentellen Ästhetik einfacher räumlicher Formenverhältnisse.

1894
EISLER, RUDOLF. Die Weiterbildung der Kant'schen Aprioritätslehre bis zur Gegenwart.
KIESOW, FRIEDRICH. Beiträge zur physiologischen Psychologie des Geschmacksinnes.
KÜHN, VICTOR. Kurze Darstellung und Kritik der praktischen Ideen Herbarts von Standpunkt religiöser Heteronomie.
SCHMID, SEBASTIAN. Schopenhauers Willensmetaphysik in ihrem Verhältnis zu neueren Ansichten über den Willen.
SMITH, WILLIAM G. Zur Frage der mittelbaren Association.
TURBAN, THEODOR. Das Wesen des Christentums von Ludwig Feuerbach.

1895
HELLER, THEODOR. Studien zur Blinden-Psychologie.
MENTZ, PAUL. Die Wirkung akustischer Sinnesreize auf Puls und Athmung.
THIÉRY, ARMAND. Über geometrisch-optische Täuschungen.

1896
BECK, PAUL. Die Substanzbegriffe in der Naturwissenschaft.
BON, SIEGFRIED. Grundzüge der wissenschaftlichen und technischen Ethik.
FRIEDRICHS, MAX. Der Substanzbegriff Spinozas neu und gegen die herrschenden Ansichten zu Gunsten des Philosophen erläutert.
GRIEVE, ALEXANDER. Das geistige Princip in der Philosophie Thomas Hill Greens.
JOVANOVITSCH, MILEVOJE. Die Impersonalien: Eine logische Untersuchung.
JUDD, CHARLES H. Über Raumwahrnehmungen im Gebiet des Tastsinns.
STRATTON, GEORGE. Über die Wahrnehmung von Druckänderungen bei verschiedenen Geschwindigkeiten.

1897
ARRER, MAX. Über die Bedeutung der Convergenz- und Accommodationsbewegungen für die Tiefenwahrnehmung.
ROSTOSKY, PAUL. Über funktionelle Beziehungen beider Gehörorgane.
SHERMAN, FREDERIC DAVID. Über das Purkinje'sche Phänomen im Centrum der Netzhaut.
TAWNEY, GUY ALLAN. Über die Wahrnehmung zweier Punkte mittelst des Tastsinnes, mit Rücksicht auf die Frage der Übung und die Entstehung der Vexirfehler.

1898
MÜLLER, ROBERT. Über Raumwahrnehmung beim monocularen indirecten Sehen.
WEYER, EDWARD MOFFAT. Die Zeitschwellen gleichartiger und disparater Sinneseindrücke.
WINTZER, WILHELM. Die ethischen Untersuchungen Ludwig Feuerbachs.

1899
JASPER, JOSEPH. Leibniz und die Scholastik.
MOSCH, ERICH. Zur Methode der richtigen und falschen Fälle im Gebiete der Schallempfindungen.
RADOSLAWOW-HADJIDENKOW, ZWETAN. Untersuchungen über das Gedächtnis für räumliche Instanzen des Gesichtssinns.
SCHWABE, PAUL. Michel de Montaigne als philosophischer Charakter.

1900
ALECHSIEFF, NICOLAUS. Reactionszeiten bei Durchgangsbeobachtungen.
HELLPACH, WILLY. Die Farbenwahrnehmung im indirecten Sehen.
SAVESCU, MICHAEL. Die Gefühlslehre in der neuesten französischen Psychologie.
SCOTT, WALTER. Die Psychologie der Triebe historisch-kritisch betrachtet.
ZEITLER, JULIUS. Tachistoskopische Untersuchungen über das Lesen.

1901
HÄNIG, PAULI. Zur Psychophysik des Geschmackssinnes.
LINKE, PAUL. D. Hume's Lehre vom Wissen.
VON VOSS, ROBERT. Über den Begriff der Erkenntnis insbesondere der intuitiven bei Spinoza.

1902
BARGMANN, HERMANN CHRISTIAN. Der Formalismus in Kants Rechtsphilosophie.
BAUCH, A. P. J. Die Psychologie Arthur Schopenhauers, dargestellt als Lehre von den Motiven.
DÜRR, ERNST. Über das Ansteigen der Netzhauterregungen.
ENDLER, RICHARD. Kants physische Monadologie im Verhältnis zur Philosophie und Naturwissenschaft der Zeit.
HENNIG, MAX. A. E. Biedermanns Psychologie der religiösen Erkenntnis.
SEYFERT, RICHARD HERMANN. Über die Auffassung einfachster Raumformen.
SQUIRES, WILHELM HARDER. Jonathan Edwards und seine Willenslehre.

1903
GENT, WERNER. Volumenkurven bei Gefühlen und Affekten.
HOFFMAN, HEINRICH. Die Leibniz'sche Religionsphilosophie in ihrer geschichtlichen Stellung.
KÖHLER, JOHANNES. Der simultane Farben- und Helligkeitskontrast mit besonderer Berücksichtigung des sog. Florkontrastes.

1904
FREUDENREICH, HANS. Fechners psychologische Anschauungen.
GUSTI, DEMETRIUS. Egoismus und Altruismus. Zur soziologischen Motivation des praktischen Wollens.
KRUMBHOLZ, FRIEDRICH. Schleiermachers Weltanschauung in den Monologen und die literarischen-philosophischen Voraussetzungen dazu.

PETERS, WILHELM. Die Farbenempfindung der Netzhautperipherie bei Dunkeladaptation und konstanter subjektiver Helligkeit.

1905

BERGEMANN, ROBERT. Reaktionen auf Schalleindrücke nach der Methode der Häufigkeitskurven bearbeitet.

KOBILECKI, STANISLAUS. Über die Wahrnehmbarkeit plötzlicher Druckveränderungen.

MITZSCHERLING, M. A. F. Die Farbenkurve bei Reduktion auf gleiche Helligkeiten.

OLSHAUSEN, WALDEMAR. Friedrich von Hardenbergs (Novalis) Beziehungen zur Naturwissenschaft seiner Zeit.

QUANDT, JOHS. WILHELM. Bewusstseinsumfang für regelmässig gegliederte Gesamtvorstellungen.

REUTHER, FRITZ. Beiträge zur Gedächtnisforschung.

RICHTER, OTTO. Kants Auffassung des Verhältnisses von Glauben und Wissen und ihre Nachwirkung besonders in der neueren Theologie.

SPEARMAN, CHARLES. Die Normaltäuschungen in der Lagewahrnehmung.

1906

BÜCHNER, MAX. Über das Ansteigen der Helligkeitserregung.

BODE, RUDOLF. Die Zeitschwellen für Stimmgabeltöne mittlerer und leiser Intensität.

KAFKA, GUSTAV. Über das Ansteigen der Tonnerregung.

KLEMM, OTTO. G. B. Vico als Geschichtsphilosoph und Völkerpsycholog.

PFEIFER, RICH. ARWED. Über Tiefenlokalisation von Doppelbildern.

REISS, JOSEF. Kritik der Willensphänomene des Privatrechts.

1907

BERLINER, BERNHARD. Der Anstieg der reinen Farbenerregung im Sehorgan.

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VOLKELT, HANS. Über die Vorstellungen der Tiere: Ein Beitr. z. Entwicklungspsychol.
WESTPHAL, WILLY. Untersuchung der sphygmographischen und pneumographischen Symptome bei Wahlreaktionen.

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LOHNERT, KARL. Untersuchungen über die Auffassung von Rechtecken.
SANDER, FRIEDRICH. Elementarästhetische Wirkungen zusammengesetzter geometrischer Figuren.

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KOBELT, WERNER. Untersuchung über die Farbenunterschiedsempfindung bei Schulkindern.

1915

BUCHHOLZ, GERHARD. Über die Beeinflussung tachistoskopischer Auffassung durch vorangehende Eindrücke.
BURGER, HERMANN. Über den Einfluss der Rhythmisierung von Eindrücken auf den Umfang des Bewusstseins.
GIESE, FRITZ. Untersuchungen über die Zöllnersche Täuschung.
TOPCIU, TRAJAN. Aufeinanderfolge zweier einfacher Reaktionen mit rechter und linker Hand bei Variation des Reizintervalls.

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MÜLLER, PAUL. Verlauf einer vorbereiteten Willensbewegung.
PAULSSEN, BERTA. Einfache Reaktionen bei Variation und rhythmischer Gliederung der Vorperiode.

An attempt has been made by 'inspection' of the titles of the theses and their authors' names roughly to classify the subject-matter investigated and the nationality of the students. While such classification can not be done without errors, the approximation is sufficiently

accurate to show major trends. Many of the theses were concerned mainly with philosophy. The frequencies of theses concerned with psychological and philosophical subjects for successive periods follow: 1875–80, two psychological and 10 philosophical; 1881–90, 27 and 22 respectively; 1891–1900, 32 and 19; 1901–10, 31 and 19; 1911–20, 24 and 0. From these data it is seen that the psychological work did not get under way until the early eighties and that from then on psychology predominated over philosophy. The ratio between psychological and philosophical theses is approximately 3 to 2 for each period after 1880 except the last, in which all theses were psychological.

Further analysis of the psychological theses reveals that about 70% can be classified as *sensation and perception* in subject-matter, 9% as *method*, 10% as *attention and feeling*, and most of the rest as scattered between *action*, *association*, and *memory*. Sub-groupings under *sensation and perception* include *vision*, 28% (of total psychological theses); *audition*, 23%; *touch*, 5%; *time-sense*, 8%; and the rest divided between *taste* and *method*.

Inspection of the names led to the following classification of nationalities: 136 German (or Austrian) students; 14 American; 10 English; 13 Balkan (from Bulgaria, Roumania, etc.); 6 Polish; 2 French; 2 Danish; 3 Russian. For the first 7 years and from 1896–1900 the Germans were fewer in numbers, but during each of the remaining quinquennia to 1916 there were about 20 names. Both the Balkan and the English students were fairly uniformly distributed through the eight periods, but nearly all (12 of the 14) Americans took their degrees between 1886 and 1900.

A CRITICAL EXAMINATION OF THE INVESTIGATIONS OF AUDITORY ACTION CURRENTS

By George Kreezer, Cornell University

For many years a controversy has existed between the adherents of the 'place' and of the 'frequency' theories of hearing. In the 'place' theories it is supposed that the neural correlate of the various distinguishable frequencies of sound consists of impulses in different nerve fibers (terminating at different places on the basilar membrane); in the 'frequency' theories the various stimulus frequencies are thought of as retained in the neural response. With the development of techniques for detecting action-currents in nerves, attempts were, naturally, early made to get a direct decision of the question by the application of this technique to the auditory nerve. But the earlier experiments were inconclusive, apparently on account of the lack of recording instruments that could respond at relatively high frequencies and still be sensitive enough to detect the action-currents. The development of electrical amplification technique with the advent of radio made it possible, however, to use instruments which though in themselves less sensitive were capable of responding at higher frequencies. So the telephone, which had early been in common use for determining the frequency of tetanic muscular contractions, was turned to again, but it now served, in conjunction with powerful amplifiers, as an indicator of the frequency of auditory action currents. As a result of such experiments with telephone and amplifier, it has been found that currents led off from the auditory nerve of the cat in response to sound stimuli, and conducted through amplifier and telephone, gave rise in the telephone to tones of the same pitch as the sounds serving as stimuli. This result, in conjunction with certain experimental checks, has led many of the recent investigators to the conclusion that the fluctuating currents detected were auditory action currents, and therefore, that impulses in the acoustic nerve correspond in frequency with the sound stimulus.

This conclusion is of such great theoretical and practical importance that it is obvious that the grounds upon which it rests must be

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very carefully scrutinized. The various investigations have not all been made under identical conditions; and the logic underlying the interpretations of the results has not always been made explicit. It seems desirable, therefore, to bring together the results of all of these investigations, along with certain other relevant facts, in order to determine just how substantial is the logical and experimental groundwork upon which rests the conclusion mentioned above. That is the purpose of the present paper. The general questions which we wish to answer are: (1) Does any single investigation provide conclusive evidence that the microphonic effect which was detected is due to action currents of the acoustic nerve? (2) To what extent do the different investigations consider the same problems and obtain corroborative results? Our procedure will be first, to review some of the fundamental facts concerning the electrical properties of tissues, then to itemize and analyze various microphonic phenomena which might be involved in investigations of the type being considered, and finally, with these considerations in mind, to pass to an examination of the various experimental investigations that have been made.

The Electrical Properties of Tissues

As we shall later see, whether certain microphonic phenomena may be expected to occur will depend on whether the tissues possess certain essential electrical properties. We wish, therefore, to recall some of the basic facts concerning the electrical properties of tissues and the way these may change under various conditions.[1]

Almost all of the electrical properties found in non-living materials are exhibited under some circumstances by living tissues. Experiments show that living tissues conduct direct currents and that they must, therefore, be designated as having a certain conductance (or resistance). Experiments with alternating currents of various frequencies show that the tissues also possess capacity. These properties may be measured by methods like those used with non-living materials. The capacity and resistance are found to vary for different kinds of tissue and for the same tissues under different conditions. Experiments show an apparently close relation between the functional and electrical properties of the tissues; consequently much of the experimental work upon the changes in the electrical properties under different conditions represents attempts to use such measures as indicators of the functional state of the tissues. The capacity and resistance are also found to vary with changes in the frequency of the alternating current being conducted. Different conventions are current for representing the manner of combination of the resistance and capacity possessed by the tissues. For repre-

[1] M. Gildermeister, Passiv elektrische Erscheinungen, *Handb. d. normalen u. pathol. Physiol.*, 8 (2), 1928, 657-681.

senting the passage of direct currents through them, it is customary to regard the tissues as equivalent to a particular resistance and capacity in parallel, as for a leaky condenser. For representing the properties of a tissue in transmitting alternating currents, it is often more convenient to regard the tissue as equivalent to a resistance and capacity in series (and to specify the changes that occur in this capacity and resistance at different frequencies). It is of course always possible, where the passage of alternating current is involved, to determine, by means of an appropriate formula, the values for a capacity and resistance in parallel which will be equivalent to a particular resistance and capacity in series. In terms of the actual structures and phenomena in the tissues responsible for its capacity, a given stretch of tissue is more appropriately represented as a group of condensers in series than as a single condenser with the measured capacity. Every place where there occurs a limiting membrane which is responsible for the piling up of ions may be regarded as corresponding to a new dielectric layer between two condenser plates. In terms of this view, the actual capacity possessed by the 'individual condensers' would be far greater than the measured value obtained for a whole stretch of tissue which is equivalent to a number of such 'unit-condensers' in series.[2]

The tissues also possess characteristics which may be likened to the electromotive force generated by batteries, though the nature of the fundamental phenomena responsible for them is still a matter of theory. If two places on a particular kind of tissue or on different tissues are connected in a circuit containing a galvanometer, a current may pass through the instrument between the two points. Such currents are called demarcation or injury currents, and the potential differences upon which they depend may be designated as injury or resting potential differences.[3] The term injury is used since it has been believed that if each of two places on the same tissue are completely uninjured, there will be no potential difference between them, although it is often difficult experimentally to prepare a given tissue so that two places on it will show no such resting potential difference. Hoffman,[4] for example, reports that in experiments made for measuring the injury currents of muscle tissues, marked differences in the magnitudes of the currents were found even though extreme care was used to prepare the specimens examined in exactly the same way. The maximum injury potential differences obtainable are different for different tissues and for the same tissues under different conditions. The maximum potential difference occurs when one point has been maximally injured (killed) and the other is completely uninjured. If both are somewhat injured the potential difference between them will naturally be less. In nerve tissue the maximum injury potential difference obtainable is of the order of 0.026 v., though its magnitude varies somewhat for different nerves and in different animals. The injured part of the tissue is negative relative to the uninjured part.

[2] *Ibid.*, 692 f.

[3] L. Hermann, *Lehrbuch der Physiologie*, 13 ed., 1905, 119; *Handb. d. Physiol.* 2 (1), 1879, 144-154; P. Hoffman, *Handb. d. normalen u. pathol. Physiol.*, 8 (2), 1928, 703-708, 738-741; R. S. Lillie, *Protoplasmic Action and Nervous Action*, 1923, 299-310; W. J. V. Osterhout, Electrical phenomena in the living cell, *The Harvey Lectures*, 25, 1931, 169-181.

[4] Hoffman, *op. cit.*, 704 f.

These resting potential differences may be produced between two places on a tissue by differences in temperature,[5] or of pressure,[6] as well as by differences in injury.

Probably related to the injury currents are the action currents or potentials.[7] As a wave of excitation passes along a tissue, it is accompanied by a wave of decreased potential, so that the active place on the tissue becomes negative relative to other points. That there is some basic connection between the two phenomena is indicated by the fact that the action and the injury potential differences are of about the same magnitude. It is by means of these action potentials that so much valuable information about the functioning of nerves and muscles has been obtained, and it is the hope of obtaining similar information about the auditory nerve that has led to the various investigations to be considered in this paper.

Microphonic Phenomena

The essential procedure in the various investigations of auditory action currents consists in exposing the auditory nerve in an appropriate experimental animal, in most investigations the cat, and in placing the active electrode on or near the auditory nerve and the other electrode on the nerve or, more generally, on nearby muscle or brain tissue. The animal is of course previously placed under an anaesthetic and usually decerebrated so that there will be no movements to interfere with subsequent experiments. The electrodes are connected to an amplifier either over a transformer or directly to grid and filament of the first tube, and any currents which may be led off in this way, when sound stimuli are sent into the cat's ear, are amplified and finally passed through a telephone or a galvanometer. In the investigations to be considered here, these currents were found to be of the frequency of the stimulating sound waves. So we shall refer to this phenomenon as the 'bio-microphonic effect' or simply as the 'microphonic effect.' A number of possible ways in which this microphonic effect may have been produced will be discussed in the present section. They will be referred to as 'microphonic phenomena.' Following is a list of the ones that we shall consider here: (*1*) Microphonic effects dependent on induction; (*2*) Microphonic effect of the tubes; (*3*) Vibration of electrodes by sound waves from the air to produce: (*a*) variation of the resistance in the input circuit, (*b*) varia-

[5] *Ibid.*, 708; cf. also W. Pauli and J. Matula, Der Thermostrom des Muskels, *Arch. f. d. ges. Physiol.*, 163, 1916, 355-383.

[6] Hoffmann, *op. cit.*, 708; cf. also J. de Meyer, Sur la dualité de la réaction électrique des systèmes musculaire, *C. r. de la Soc. de Biol.*, 83, 1920, 301; De Meyer, De la superposition des courants d'action et de déformation dans les muscles, *Arch. internat. de physiol.*, 16, 1921, 193-227; H. Lehmann, Über den Willkürversuch du Bois-Reymonds, *Arch. f. d. ges. Physiol.*, 207, 1925, 316-319.

[7] Hoffmann, *op. cit.*, 709-737; 741-758.

tion of the capacity in the input circuit; (*4*) Vibration of the electrodes by sound waves transmitted from the labyrinth: (*a*) and (*b*) as under (*3*); (*5*) Fluctuating potentials (which produce a fluctuating potential difference between the electrodes): (*a*) generated in the tissues by the passage of sound waves, (*b*) generated in the labyrinth by the passage of sound waves, (*c*) generated in the auditory nerve by the passage of nerve impulses.

(*1*) *Induction.* An obvious artifact to be avoided in the investigations being examined here is that due to induction effects. Fluctuating electric or magnetic fields which vary synchronously with the stimulating sound waves may induce corresponding fluctuations of current or potential in the amplifier and so give rise to telephone tones of the same pitch as the stimuli. This danger is the greater, the more sensitive the amplifying system; for the fluctuating currents or potentials induced may then be very minute and yet be detected. Hence all of the experimenters who have reported upon the 'biomicrophonic effect' have avoided the use of electrical sources of sound stimuli and have relied upon air sources alone.

But the presence of fluctuating electric and magnetic fields of like frequency with the sound-stimulus is not limited to those that may be produced by electrical sources of sound. If anywhere in the region that may be reached by the stimulus-waves there is electrical apparatus which permits the sound waves to produce vibration of a conducting wire in a magnetic field, or the variation of the reluctance of part of a magnetic circuit (as by a vibrating membrane near the ends of telephone magnets), or the production of fluctuating currents synchronized with the sound waves, or the vibration of a charged body, then fluctuating electric and magnetic fields may be produced.

Amplifiers are quite generally provided with shielding to exclude disturbances from the surroundings. But even where present, such shielding designed for general purposes cannot be *assumed* to have been adequate to prevent fluctuating fields, such as those mentioned above, from inducing microphonic effects in the amplifier. Shielding is a relative matter. Whether it is adequate in a given case depends upon the strength and frequency of the fluctuating fields, the characteristics of the shielding system (nature of the material, its thickness and form, presence of slits and joints, all of which determine the ease with which eddy currents may flow), the degree of amplification, and

the characteristics of the circuit which is being protected, as, for example, the input circuit of the amplifier.[8]

To avoid the effects of fluctuating fields produced in the ways mentioned above, it is of course desirable that all electric apparatus that might give rise to such fields be removed from the vicinity of the stimulus-room, and that appropriate shielding against fluctuating electric and magnetic fields be provided. But even with such precautions, the exclusion of induction effects cannot be considered assured until appropriate tests have indicated this to be true. Such tests are as necessary as is testing the soundproofing of the room containing the amplifier to assure exclusion of the microphonic effect of the tubes. Such tests, to be adequate, must also take account of the fact that the characteristics of the input circuit may determine the intensity of the effects that might be produced in it by induction. During the tests, therefore, the input circuit must have magnitudes of resistance, capacity and inductance comparable with those existing in the circuit during the experiments, when the animal tissues between the electrodes provide certain of these magnitudes.

(*2*) *Microphonic action of the tubes.* The vibration of the amplifier tubes by the sound waves may produce movements of the grid and filament relative to each other, altering the grid-filament capacity, and thus produce what is called the microphonic effect of the tubes. This phenomenon, which is relatively easy to detect, due to its favoring of certain frequencies and its independence of the characteristics of the input circuit, seems not to have been a factor in any of the experiments and so need not be further discussed.

(*3*) *Vibration of the electrode—sound waves from the air.* Microphonic phenomena due to the vibration of the electrode by air-waves (air vibration phenomena) may be of two kinds: (*a*) those dependent upon the variation of the resistance in the input circuit (resistance-microphone phenomena); and (*b*) those dependent upon the variation of the capacity of the input circuit (condenser-microphone phenomena).

[8] J. H. Morecroft and A. Turner, The shielding of electric and magnetic fields, *Proc. Inst. Radio Engineers*, 13, 1925, 477-505. The significance of the characteristics of the circuit is indicated by the formulae for the flow of alternating currents in various kinds of circuit: see, *e.g.*, V. Karapetoff, *The Electric Circuit*, 2nd ed., 1912, 60-80.

(*a*) *Variation of resistance of input circuit.* The model for resistance-microphone phenomena is furnished by the carbon microphone.[9] Direct current from a battery flows through a resistance provided by carbon granules lying loosely behind the telephone transmitter membrane, then through the line to a telephone or transformer and back again to the battery. Vibration of the transmitter membrane by sound waves changes the contact among the carbon granules and thus the resistance of the circuit. Corresponding fluctuations in the direct current through the circuit results. For such a microphonic effect to be produced, it is apparent that there must be a source of direct current, a complete circuit around which the direct current may flow, and a means by which pressure variations of the sound waves may produce corresponding variations in the resistance of the circuit.

When these considerations are applied to the experiments being reviewed here, it is evident that such a 'resistance-microphone' phenomenon might occur in the experiments in which the electrodes are connected to the amplifier over a transformer. The source of direct current might be provided by an injury potential difference of the tissues in contact with the electrodes; the closed circuit by the series path from one electrode through the tissues to the other electrode, through the primary of the transformer and back again to the first electrode. The circuit would contain an inductance and capacity in series due to the inductance of the transformer primary and the capacity of the tissues. The tissues must be regarded here as a resistance and capacity in parallel, the continuous current passing by way of the resistance branch. The variations in resistance might be produced by the vibration of the active electrode by sound waves reaching it either directly from the air, or coming through the tissues from the cochlea or other parts of the labyrinth. The latter case we shall consider further on. In a microphonic circuit of the sort described, it is apparent that the amplitude of the current fluctuations produced by the vibrating electrode will depend on all of the electrical dimensions of the circuit: the resistance, the capacity, and induction of the total circuit, as well as the voltage of the steady electromotive force and the amplitude of the fluctuations in resistance.

(*b*) *Variation of capacity of input circuit.* In the investigations in which the electrodes are connected directly to the grid and filament and not over a transformer, a resistance-microphonic effect would not be possible, on account of the absence of one essential for such a phenomenon, a continuous circuit for direct current. But an analogous phenomenon, dependent on the variation of the capacity of the input circuit, would still be possible. The model for this phenomenon is furnished by the condenser microphone.[10] It depends on the principle that if a charged condenser connected to an external circuit is subjected to vibratory pressure changes producing corresponding variations in capacity (due to variation in the separation of the plates or in the dielectric constant of the medium between the plates), then an alternating electromotive force will be set up in the circuit. This will occur whether the external circuit consists of an inductance,

[9] A. B. Wood, *A Textbook of Sound*, 1930, 382-402; G. D. Shepardson, *Telephone Apparatus*, 1917, 89-99. For derivation of formulae expressing the dependence of microphonic currents on the characteristics of the circuit for commercial circuits, see Shepardson, *op. cit.*, 106-113.

[10] Wood, *op. cit.*, 395-398; I. B. Crandall, *Theory of Vibrating Systems and Sound*, 1926, 249 f.

resistance, or another capacity (as provided, for example, by the grid-filament capacity of a vacuum tube), though the magnitude of the alternating voltage across the terminals of the external circuit will depend upon its properties as well as upon those of the rest of the circuit. In terms of the conditions of the present experiments, the vibrating (active) electrode can be considered one plate of a condenser, and the tissues the other plate. The electromotive force for charging the 'condenser' is represented by the injury potential difference, as in the case above. Vibration of the electrode may produce fluctuations of the capacity and corresponding fluctuating currents through the transformer or a fluctuating potential difference between the grid and filament of the first tube, if no transformer is used. It is also possible to regard the condenser as provided by the entire stretch of tissue between the two electrodes. As pointed out earlier, the capacity of the tissues may be thought of as corresponding to the capacity of a group of condensers in series with each other, the occurrence of such a series being due to the successive membranes at which a piling up of ions occurs. So the fluctuations in capacity of the input circuit might be produced in the tissues themselves by the passage of the sound waves through them. In either case, the amplitude of the fluctuating electromotive force produced will depend on all of the electrical dimensions of the input circuit: the resistance, capacity, and inductance in the total circuit, the voltage of the injury potential difference and the amplitude of the capacity changes produced by vibration of the electrode or passage of the sound waves through the tissues.

(4) *Vibration of the electrode—sound waves from labyrinth.* The sound waves responsible for setting the electrode into vibration may, as mentioned above, come from two sources, either directly from the air or from the cochlea (or other parts of the labyrinth) by way of any tissues intervening between the walls of the labyrinth and the electrode. In many of the investigations, the active electrode rests directly against the wall of the labyrinth (*e.g.* against the medial surface of the petrous bone, when the electrode is around the auditory nerve). As is well known, the bones of the middle ear are adapted for transforming the sound waves striking the ear drum into vibrations of greater force, but correspondingly reduced displacement-amplitude. Some of the energy of the resultant vibrations in the liquids of the cochlea might be expected to be transmitted through the bony case of the cochlea, and from there further on through the tissues adjacent to the petrous bone. Bone is a good conductor of sound. The tissues are aqueous in nature, and water too is a good conductor of sound. Sound-conductance in the tissues would, of course, be less than in the case of the homogeneous liquid since, in the case of the tissues, the additional materials present in the form of colloids and suspensions would more quickly damp out the vibrations. But just how soon this would occur for a sound of given frequency

and energy could be determined only experimentally. Another possible path of transmission of vibrations from the cochlea that has been suggested is that by way of the auditory nerve out through the inner auditory meatus. This too is a possible path, although the path by way of the bone-walls of the cochlea (or other parts of the labyrinth) is probably of much lower acoustic impedance.

As for the transmission of sound waves in the brain tissues, it seems unlikely that this would occur with equal facility in all directions from the petrous bone. For such equality might be expected only in a homogeneous medium. The acoustic conductivity in the different directions must depend on the same factors, such as viscosity of the medium, reflection, refraction, diffraction, and interference, which determine the transmission of sound in general.[11] The marked differences in the texture and structure of the tissues in the various directions about the medial surface of the petrous bone is probably coupled, therefore, with corresponding differences in acoustic impedance. It is conceivable, for example, that the sound waves will be propagated relatively better along paths of greater homogeneity, such as along the length of nerve trunks and along the paths followed by blood vessels. At any rate, in the absence of experimental data on the distribution of acoustic impedance in the various parts of the brain, it is not possible to assume that differences do not exist. If sound vibrations are transmitted through the bony walls of the labyrinth and further along through the tissues, then such motions would be imparted to an electrode resting against the bone or further along in the tissues. Both of the microphonic phenomena discussed in the last section might then occur. The magnitude of the resultant microphonic effects would, however, depend here on two additional factors: the energy of the sound waves in the liquids of the labyrinth and the acoustic impedance of the path from these liquids to the electrode.

(*5*) *Fluctuating potentials:* (*a*) *Generated in the tissues by the passage of sound waves.* It is possible that the fluctuating potentials underlying the 'bio-microphonic effect' may have been produced in the tissues themselves by the passage of sound waves. In a previous section, we have mentioned the possibility that such a phenomenon may occur as a consequence of changes in the capacity of the tissues by sound waves. But we wish to suggest here the possibility of such

[11] Wood, *op. cit.*, 229-335.

a phenomenon occurring on the basis of certain empirical evidence and independently of any hypothesis as to the mechanism underlying the phenomenon.

(1) Osterhout and Hill, as a result of experiments upon the sea-plant Nitella report that mechanical stimuli which travel down a cell are accompanied by a wave of decreased potential, similar to that which accompanies a wave of excitation. According to a summary statement of the authors: "Mechanical stimulation of Nitella often produces responses resembling propagated negative variations but traveling faster and going past a killed spot. They appear to result from a mechanical disturbance traveling along the cell and stimulating each spot it touches (*i.e.* the stimulus itself travels). They are called mechanical variations to distinguish them from propagated negative variations. . . . The all or none law does not apply to incomplete mechanical variations, for the response varies with the strength of the stimulus."[12]

(2) As mentioned earlier, De Meyer has reported the production of potential changes of considerable magnitude as a result of mechanical distortion of muscles.[13] When resting muscle is deformed from the outside in without injury, potential differences (as high as 1.5 mv.) occur between it and neighboring points. The deformation current appears without latency and is approximately proportional to the deformation. The greater the deformation, the more positive is the place.

(3) A recent report by Keller is also of interest in this connection.[14] Keller, in commenting upon the significance of the microphonic effect reported by Wever and Bray gave some reasons why he believed that it was not due to action currents. He said he had found similar phenomena in the vagus and depressor nerves which his observations indicated were not a result of true action currents. He found that when the experimental animal breathed noisily, the acoustic frequencies corresponding to the sounds would be reproduced in the curves of the oscillograph used for recording the action currents of the nerve. (Mechanical or acoustic transmission to the amplifier he considered to be excluded since the nerve hung loosely on the electrodes and the amplifier was in a soundproof chamber.) He found that the effect could also be obtained if a silk thread, soaked in Ringer's solution, was placed around the animal's trachea and the end of the thread placed on the electrodes, or if the string was tied to a monochord and the monochord struck. He suggested that the effect might be due to electrostatic displacements produced by the vibrations and that the arrangement might be designated as a thread-microphone. Such phenomena, he thought, were probably responsible for the microphonic effect that had been detected in the auditory nerve.

The results of these investigations indicate that mechanical changes in tissues may produce corresponding variations in electrical potential. It is possible, then, that in the investigations made of the 'bio-microphonic effect,' the variations in pressure involved in the

[12] W. J. V. Osterhout and S. E. Hill, Electrical variations due to mechanical transmission of stimuli, *J. Gen. Physiol.*, 14, 1931, 485.

[13] See footnote 6.

[14] Ch. J. Keller, Über ein merkwürdiges Phänomen an Niederfrequenzverstärkern, *Z. f. Biol.*, 91, 1931, 346-348.

transmission of sound waves through the tissues in the neighborhood of the active electrode may have given rise to corresponding fluctuations of electric potential, and so have led to a microphonic effect in the telephone.[15]

(*b*) *Generated in the labyrinth.* The fluctuating potentials responsible for the 'bio-microphonic effect' may have been produced in the cochlea or some other part of the labyrinth. Whatever the nature of the underlying mechanism, the intensity of the resultant microphonic effect would depend, presumably, upon the energy of the sound vibrations occuring in the liquids, the possibilities for changes in the concentration of ions, and the electrical impedance of the path between the place of origin of the fluctuating potentials and the active electrode.

(*c*) *Generated in the auditory nerve.* Fluctuating potentials generated in the auditory nerve is the phenomenon which it was the purpose of the various investigations to discover. If it can be demonstrated that the 'bio-microphonic effect' detected is in fact due to action potentials of the auditory nerve, then the fact that such an effect occurs is obviously of significance for auditory theory. A microphonic effect due to auditory action currents would depend presumably upon the energy of sound waves occuring in the cochlea, the functional condition of the auditory nerve, and the impedance of the electrical pathway between the active point on the nerve and the active electrode.

It has been apparent that every one of the various microphonic phenomena which we have examined has its own peculiar conditions. Furthermore, the technical and accidental conditions of the various investigations cannot be regarded as identical. There are, for example, differences in the degree of amplification used, differences in the way the electrodes are connected to the amplifier (and therefore differences in the characteristics of the input circuit), and, finally, differences in the electrical properties of the tissues involved. The last, *i.e.* the electrical properties of the tissues, cannot readily be controlled, and there is no indication in most of the investigations of any attempt to control or even to specify them. There is no reason,

[15]Whether electrical potentials generated in or near the auditory nerve in this way would be capable of stimulating the nerve to conduct auditory-nerve impulses, that is to say, impulses that might mediate experiences of sound, is a separate question and need not be discussed here. The point involved here is that such fluctuating electrical potentials would not be action potentials in the customary sense of the term and so could not be used as indicators of the nature of the impulses in the auditory nerve.

then, for expecting that the same microphonic phenomena will occur in the various investigations. Consequently every investigation must be examined as a unit in itself; and even though some particular phenomenon may have been excluded as a factor in one investigation (or experiment), it does not follow that it has been absent from another, unless this fact is specifically stated and demonstrated. It is also apparent that the microphonic effect which occurs at a given time cannot be assumed to be due to but a single one of the conceivable microphonic phenomena. It is possible in a given case that a number of microphonic phenomena may be involved and that the effect detected is a resultant of the component effects of many of them; furthermore, the relative share contributed by each of the phenomena involved to the resultant effect may shift and change as the conditions of the experiment change. With these considerations in mind, let us now examine the various investigations to see whether any of them provides conclusive evidence that the 'bio-microphonic effect' or some fraction of it is due to action currents of the auditory nerve.

Investigations of the Bio-Microphonic Effect

The investigations that we shall consider here are those of Wever and Bray,[16] Adrian,[17] Adrian, Bronk, and Phillips;[18] Wever;[19] Davis and Saul;[20] Hughson and Crowe;[21] Rademaker and Bergansius;[22] and Kreezer and Darge.[23] We shall take them up as nearly as possible in the order of their publication.

Wever and Bray. As a result of their experiments, Wever and Bray came to the conclusion that the microphonic effect detected by them was due to action currents in the auditory nerve. Their procedure for reaching this conclusion was by a process of elimination. Every one of a number of other possible microphonic phenomena are

[16]E. G. Wever and C. W. Bray, The nature of acoustic response, *J. Exper. Psychol.*, 13, 1930, 373-387.

[17]E. D. Adrian, The microphonic action of the cochlea: an interpretation of Wever and Bray's experiments, *J. Physiol.*, 71, 1931, xxviii-xxix.

[18]E. D. Adrian, D. W. Bronk, and G. Phillips, The nervous origin of the Wever and Bray effect, *J. Physiol.*, 73, 1931, 2-3P.

[19]E. G. Wever, Impulses from the acoustic nerve of the guinea pig, rabbit, and rat, this Journal, 43, 1931, 457-462.

[20]H. Davis and L. J. Saul, Action currents in the auditory tracts of the midbrain of the cat, *Science*, 74, 1931, 205 f.

[21]W. Hughson and S. J. Crowe, Function of the round window, *J. Amer. Med. Assoc.*, 96, 1931, 2027-2028.

[22]G. G. J. Rademaker and F. L. Bergansius, Expériences sur la physiologie de l'ouie, *Arch. neerl. de physiol.*, 16, 1931, 346-349.

[23]G. Kreezer and H. Darge, Auditory action currents, *Science*, 75, 1932, 105.

considered in turn and experiments performed to determine whether it could be considered responsible for the effect. Every one in turn is judged to be excluded by the results of the experiments, and a final experiment is then made to decide between the two remaining possibilities, resulting in the conclusion mentioned above. Let us now examine these experiments more closely.

(*1*) *Induction.* The authors state that "induction effects are of first importance in the use of a sensitive electrical system if the stimulus is electrical or magnetic in nature." They have therefore avoided the use of such stimulating apparatus and were content with the use of air-sources of sound. Tuning forks too were avoided since they found "that if they are placed before the animal's ear in the ordinary way the vibration of their magnetic fields—for they seem invariably magnetized—sets up induced currents in the lead-in wires." The authors state that tests show that tuning forks as well as audio-oscillators "can be employed if the animal and the electrode wires are carefully shielded." But they conclude "it has been simpler in these preliminary experiments to use wind sources of sound."[24]

It is apparent from the authors' account of the measures they adopted to avoid induction effects that they had in mind only those induction effects which might have been due to the production of fluctuating electromagnetic fields by the sources of stimuli, and that to avoid them they have limited themselves to wind sources of sound. As has been indicated earlier in this paper, and in a recent note,[25] fluctuating fields synchronized with the sound waves might have been generated in other ways than by the stimulus-sources. Wever and Bray, in a recent note, say merely that "it should be unnecessary to state" that such effects were not involved in their experiments.[26] We fail to see, however, that the absence of such induction effects can be assured except by the use of the precautions we have mentioned in a previous section, and which involve a final test of the effectiveness of any shielding that may be in use, with proper regard for the influence of the characteristics of the input circuit upon the results of such a test. There is no indication in any of Wever and Bray's reports that such measures were taken.

[24] Wever and Bray, *op. cit.*, *J. Exper. Psychol.*, 380 f.
[25] Kreezer and Darge, *op. cit.*, 105.
[26] E. G. Wever and C. W. Bray, Kreezer and Darge on auditory action currents, *Science*, 75, 1932, 267.

(*2*) *Microphonic effect of amplifier tubes.* The microphonic effect of amplifier tubes seems to have been effectively avoided by placing the amplifier in a soundproof room and the preparation in another room about 50 ft. distant.

(*3*) *Mechanical action of the electrodes.* In explaining what is meant by the phrase "the mechanical action of the electrodes," the authors say: "It is easy to imagine that the pressure variations of the air cause slight movements of the electrode wires in the tissues, and that these movements produce a change in resistance between the electrodes or a change of inter-electrode potential. If the electrodes are placed on dissimilar tissues an inter-electrode potential and hence a more or less steady direct current is doubtless present, and changes of the sort suggested would act on the amplifier so as to produce an audible signal corresponding to the stimulus."[27] A number of experiments were performed to test this possibility.[28] The authors found that the effect was eliminated: (1) when the electrodes were placed in other tissues, away from the acoustic region (with certain exceptions); (2) upon death of the animal; (3) as a result of the destruction of both cochleas; (4) upon restriction of the blood supply to the head; and (5) as a result of a simultaneous polarizing current sent through the tissues between the two electrodes (active electrode around the nerve and indifferent electrode on cerebral tissue). They considered this evidence "effectively to eliminate mechanical action at the electrodes, and to indicate the cochlear origin of the effects."

But in the case of no one of these checks is there any assurance that the conditions which produced loss of the microphonic effect did not also produce changes in the electrical properties of the tissues upon which the air-vibration phenomenon depends. For this microphonic phenomenon depends not only on whether sound waves from the air are available for striking against the electrodes, but also on the electrical properties of the tissues between the electrodes—the magnitude of the injury potential difference, and the capacity and resistance of the tissues. If changes occurred in these properties that would reduce *at all* the magnitude of the effect due to vibration of the electrodes, then the checks obviously would be valueless as demonstrations that air-vibration effects were not involved. In the case of all of these checks (except possibly that with the polarizing currents), a certain decrease in the total microphonic effect was to be

[27] Wever and Bray, *op. cit.*, *J. Exper. Psychol.*, 13, 1930, 381.
[28] *Ibid.*, 381-384.

expected from the elimination of the components dependent on energy derived from the cochlea; consequently if any additional decrease might be expected from changes in the factors underlying air-vibration phenomena, then the checks would lose any validity they might otherwise have had. Unfortunately, Wever and Bray present no evidence to show that such changes in the electrical properties of the tissues did not occur, an omission which deprives the checks of any claims to conclusiveness. Below is presented some evidence which indicates that the checks performed would, in fact, have been likely to alter the electrical properties of the tissues between the electrodes. To just what extent such changes occurred in the experiments of Wever and Bray, and the extent to which they determined their results could have been determined only under the specific conditions of their experiments.

In experiments of Kreezer and Darge,[29] marked differences were found between the electrical impedance of tissues in the neighborhood of the acoustic nerve and the impedance of tissues in other parts of the body such as were selected by Wever and Bray for their checks. The experimental object was a living cat which had been prepared for the experiment in the manner followed in the experiments of Wever and Bray; that is to say, the cat was placed under ether, the carotid arteries were tied, and the cat decerebrated. Measurements of impedance were made by means of Wheatstone bridge and telephone; an audio-oscillator served as a source of alternating current. To measure the impedance of the brain tissues in the neighborhood of the acoustic nerve, one electrode was placed besides the petrous bone, near the acoustic nerve, and the other electrode on the opposite side of the brain-stem, just caudal and dorsal to the ventral edge of the tentorium. Following are some illustrative measurements. At an alternating frequency of 450 cycles per sec., the impedance was 1340 ohms; at a frequency of about 800 cycles, impedance was 1310 ohms. Then one electrode was placed around the saphenous nerve in the leg, and the other under near-by skin tissue. At 450 cycles impedance was 2500 ohms, at 800 cycles it was 2450 ohms. With one electrode around the nerve, and the other in muscle tissue 1 cm. away, the impedance at

[29]The experiments upon the electrical impedance of the tissues that are referred to in the present section were all performed in collaboration with Mr. Hans Darge and will soon be published. The impedance (Z) of the tissues measured here is a function both of the effective resistance and of the capacity of the tissues. The influence of particular changes in capacity and resistance of the tissues upon the total impedance of an input circuit containing a transformer would depend, of course, upon the magnitude of the self inductance of the primary of the transformer as well as upon the capacity and resistance of the tissues. Calculations of the impedance of the total circuit could be made only if the individual values of the capacity, resistance, and self-inductance were known. It will be recalled that impedance takes the place in alternating current formulae of resistance in Ohm's law for direct currents. That is to say, the impedance of a circuit to alternating currents equals the effective voltage of the alternating electromotive force divided by the effective value of the total current in the circuit.

450 cycles was 2120 ohms; at 800 cycles, 2030 ohms. With both electrodes in muscle at a distance apart of 1 cm., the impedance at 450 cycles was 2310 ohms; at 800 cycles, 2200 ohms. With one electrode in muscle and the other in fatty tissue 4 cm. away, the impedance at 450 cycles was 3400 ohms; at 800 cycles, 3250 ohms. In these experiments Kreezer and Darge did not expect to duplicate the exact positions which the electrodes had had in the checks; for that, the descriptions are not precise enough. They wished merely to see whether, when the electrodes are placed first in the acoustic region and then in other parts of the body such as were used for the checks, any marked differences in the impedance of the tissues would be found. That such differences were found is clear from the figures reported.

The experiments of Kreezer and Darge also showed that marked changes in the impedance of the brain occur upon the death of the cat. The experimental animal was prepared as in the experiments just described, and the electrodes in the brain were located as before—one near the auditory nerve and the other on the opposite side of the brain-stem near the tentorium. The following figures, obtained in one of the experiments, are indicative of the changes occurring with death. Measurements of the electrical impedance of the tissues were made at 5-min. intervals for a period of 2½ hr. The cat breathed regularly without the aid of artificial respiration since the centers in the medulla had not been injured. The rubber tube leading to the trachea was then clamped and the cat permitted to die. The impedance to a frequency of 450 cycles just before death was 1260 ohms, at about which level it had been during the previous period of measurements. After death, the impedance rose steadily: ½ hr. after the heart stopped beating it was 1660 ohms; 1 hr. later it was 1820 ohms. A parallel rise in the impedance occurred for a frequency of alternating current of 800 cycles, at a slightly lower level of impedance.

That failure of the circulation is accompanied by changes in the electrical properties of the tissues is indicated by the results which have just been cited upon changes in impedance after the heart stopped beating. The investigations of Galleotti[30] and Von Bud[31] on the changes in the resistance and capacity of tissues with the onset of death are also relevant here. Hermann reports too a decrease in the injury current of nerves with the onset of death (more rapid for warm-blooded animals), and with fall in temperature.[32] In general, the close correlation that has been found to exist between even slight changes in the functional condition of an animal and the electrical properties of the tissues indicates that changes in the blood supply to the tissues would be likely to alter their electrical properties.[33]

The changes in these properties which might be produced by cochlear destructions would probably be dependent on any disturbances that these destruc-

[30]G. Galleotti, Über die elektrische Leitfähigkeit der tierischen Gewebe, *Zsch. f. Biol.*, 43, 1902, 301-316; 45, 1904, 65-78.

[31]G. von Bud, Permeabilität und Autolyse, *Zsch. f. Biol.*, 86, 1927, 108-114.

[32]L. Hermann, *Handbuch der Physiologie*, 2 (1), 1879, 148.

[33]G. W. Crile, H. R. Hosmer and A. F. Rowland, The electrical conductivity of animal tissues under normal and pathological conditions, *Amer. J. Physiol.*, 60, 1922, 59-106; W. J. V. Osterhout, *Injury, Recovery, and Death in Relation to Conductivity and Permeability*, 1922, 15-20; Gildermeister, *op. cit.*, 657-681; Lillie, *op. cit.*, 299-336.

tions might produce in the blood supply to the tissues of the head. Such disturbance of the blood supply becomes not unlikely when we consider the situation of the experimental animal as a result of the technique that had been followed in making it ready for the experiments. The tying of the carotid arteries and decerebration would mean that the sole blood supply to the tissues of the head and, therefore, to those included in the input circuit, would now be by way of vessels supplied by the basilar artery and the two vertebral arteries which supply it. Destruction of the cochleas could scarcely avoid rupturing the vessels supplying them, the two auditory arteries which branch off from the basilar artery. Such a rupture would drain away blood coming through the basilar and vertebral arteries and consequently disturb the distribution of blood to the regions of the head supplied by branches of the basilar artery.

Kreezer and Darge also performed experiments upon the effect of a simultaneous polarizing current on the impedance of the tissues to alternating currents. The previous arrangement used for measuring the impedance of the tissues was combined with a battery and a potentiometer to serve as a source of direct current. The two electrodes were placed in the brain-tissues as in the previous experiments described. Following are figures which represent typical results. In this list the first quantity, in volts, refers always to the voltage of the direct current supply; the second figure to the impedance of the tissues to an alternating current of 450 cycles sent simultaneously through the same tissues: 0 volts, 1360 ohms; 1 volt, 1360 ohms; 2 volts, 1420 ohms; 3 volts, 1420 ohms; 4 volts, 1600 ohms; 5 volts, 1880 ohms; 6 volts, 2070 ohms; 7 volts, 2180 ohms; 8 volts, 1930 ohms. For a setting of 0 volts made immediately after the previous measurements, the impedance was 1420 ohms. About an hour after the polarizing current had been discontinued the impedance had dropped back again to a relatively steady value of 1340 ohms. A curve of the same form was obtained for an alternating current of 800 cycles, at a slightly different level of impedance.[34] These results show clearly the marked change that is produced in the electrical properties of the tissues by a simultaneous polarizing current. In Wever and Bray's experiment it had been found that polarizing currents of from 4-8 volts were sufficient to eliminate the microphonic effect.

This account is enough to show that a constancy in the electrical properties of the tissues under the conditions of Wever and Bray's checks cannot be assumed. These checks, however, have failed to take account of the possible changes and cannot, therefore, be regarded as conclusive.

(4) *Mechanical action of the cochlea.* By the mechanical action of the cochlea Wever and Bray refer to the microphonic phenomena dependent upon vibration of the electrodes by sound waves which may be transmitted from the cochlea, rather than directly from the air. As expressed by the author, "it may be supposed that the cochlea, being especially sensitive to aerial pressure variations, picks

[34]The direct current voltage between the electrodes in these experiments is about three fourths of the applied electromotive force of the total circuit, as drawn from the potentiometer, on account of the potential difference drop in the circuit before the animal. The figures given above for the voltage are for the total circuit.

up and transmits these variations to the substance of the nerve in a mechanical fashion, and the resulting movements of the nerve against the electrode vary the inter-electrode potential and thereby produce the response."[35] Against this possibility the authors argue "that nerve substance is a poor mechanical conductor" and that "this would be especially true of the auditory nerve, since it originates in small filaments which later unite into the larger trunk emerging from the internal meatus." But as we have already pointed out, the auditory nerve is not the only path for sound waves propagated from the cochlea, or indeed, the best conducting one. Another and probably better path is provided by the bony case enclosing the liquids of the cochlea. The authors add as a further argument that "the cessation of the response on the death of the animal and on the introduction of a polarizing current into the nerve argues against mechanical action of this kind, as it seems unlikely that such action should be seriously impaired by these changes." But as has been also noted above, such changes in the animal modify certain of the electrical properties of the tissues, which are also an essential factor in microphonic phenomena dependent upon "mechanical action of the cochlea."

Three additional experiments are cited as furnishing more positive evidence against the possibility that the microphonic effect is due to "mechanical action of the cochlea:" (*a*) the persistence of the effect when an attempt was made to eliminate the inter-electrode potential; (*b*) the fact that the response is fairly well localized in certain tracts of the nervous system; and (*c*) "the cotton and film" experiment.

(*a*) *Experiments to eliminate the inter-electrode potential.* Wever and Bray point out that the microphonic phenomenon under consideration would require the presence of an inter-electrode potential and they state that "under ordinary conditions, where the two electrodes are on dissimilar tissues, such a potential is the rule. An effort was made, therefore, to reduce or eliminate such a potential as a check against the artifact suggested. In certain experiments non-polarizable electrodes of the silver-chloride type were used, one on the nerve and the other in the cerebrum. The response continued as before. Two silver-chloride electrodes were then imbedded in an insulating cement at a distance of 2 mm., and both placed in contact with the nerve." "By this means," they continue, "we sought to avoid an inter-electrode potential by having the electrodes on similar and adjacent tissue, and by the embedding cement to avoid the possibility of alterations of the distance between the electrodes through any movements in their neighborhood. The responses continued as strongly as before, but perhaps with a certain amount

[35] Wever and Bray, *op. cit.*, *J. Exper. Psychol.*, 13, 1930, 384 f.

of distortion. It occurred to us that such distortion might be the result of the reception of diphasic action currents (cat 10)."[36]

As has been noted in an earlier section, the inter-electrode potential may depend not only on the polarization of the electrodes and their presence in different tissues, but on *differences in injury, pressure, or temperature* of two places on the *same tissue* in contact with the electrodes. The means adopted by Wever and Bray for eliminating the inter-electrode potential cannot, therefore, be regarded as adequate. Its elimination must be demonstrated, not assumed. Nor does the mere imbedding of the electrodes in insulating cement guarantee the elimination of relative movement between the tissues and the electrodes. But even if these measures had sufficed to eliminate the possibility of microphonic effects due to vibration of the electrodes, the results would not demonstrate that this phenomenon was not involved in *any* of the experiments (which, we take it, is the point of the checks). The persistence of the effect after the measures taken to eliminate the inter-electrode potential, etc. could, at most, mean that other causes for the microphonic effect exist, that microphonic effects dependent upon vibration of the electrodes are not the only possible ones. Of these other possibilities there are, as shown above, a considerable number. But it would not follow that the microphonic phenomena dependent on vibration of the electrodes could then be regarded as absent from all of the experiments. For if a given phenomenon should be eliminated in certain experiments, by elimination of some of its essential conditions, it obviously does not mean that this phenomenon would also be absent when the essential conditions are not eliminated.

(*b*) *Path of transmission.* Wever and Bray cite as additional evidence against "any hypothesis of mechanical transmission" the fact that the response is fairly well localized in certain tracts of the nervous system and that "the currents can be picked up across the medulla and from the brain stem, at distances which would seem too great for mechanical action to be effective. Under the same conditions, other tissues equally distant from the nerve do not give the effects."[37] These results are of significance as checks against "mechanical action of the cochlea" only if it is true that the distances *are* "too great for mechanical action to be effective," and if the transmission of sound waves is as good to places in the tissues where the effect is not picked up as to those where it is detected. Without proof of these statements, the results reported cannot be regarded as adequate checks.

(*c*) *The 'cotton and film' experiment.* This experiment is designated by the authors as one of the best checks they have and so it merits particular consideration. It was made possible by the exceptional cases in which the response was obtainable from tissues off the nervous system. "It was found that the active electrode placed on a lump of wet cotton resting on the temporal bone, and the inactive electrode on distant muscular tissue, picked up the responses readily. But if a piece of dry collodion film were interposed between the cotton and the bone the responses disappeared. Yet if a small strand of the cotton was picked up with the forceps and carried over the edge of the film so as to make contact with the moist bone or muscular tissue beneath, the response returned (cats 11, 8). The experiment yielded the same results if both electrodes were on the cotton

[36] *Ibid.*, 385. [37] *Ibid.*, 385.

(cat 11). As the manipulation of the strand of cotton did not alter the mechanical features of the situation in any essential manner, we may conclude that the transmission concerned is electrical rather than mechanical in nature."[38]

Let us consider first the case in which the active electrode rests on wet cotton and the indifferent electrode on distant muscular tissue. The collodion film between the cotton and the tissues obviously breaks the circuit between the electrode and the tissues. The authors forget that this connection is as necessary for the microphonic phenomenon under discussion as it is for those dependent on electrical changes coming from the tissues or the cochlea. For how can variations in resistance, produced by vibration of the electrode, lead to microphonic fluctuations of current unless there is a closed circuit so that currents may flow? The provision of the moist cotton switch makes it possible, not only for electrical changes in the tissues to be detected by the electrode, but also for 'mechanical action of the cochlea' to become capable of producing a microphonic effect. To use this experiment to prove that the microphonic effect does not depend on vibration of the electrodes would be like trying to prove that the microphonic effect produced by a carbon or condenser microphone does not depend on mechanical vibrations since the effect disappears when the part of the circuit leading to the battery is broken.

When the electrodes are both on cotton, the situation is somewhat different. This variety of the check is like removing the battery from a carbon or condenser microphone circuit and connecting together the two leads (by a bit of moist cotton). Obviously, under such conditions, the microphonic effect produced by the sound vibrations will disappear. With both electrodes on the cotton there is no way by which injury currents can provide the inter-electrode potential which is necessary in order for 'mechanical action of the cochlea' to be effective. The effect which appears when the connection is made with the tissues beneath, by the moist cotton switch, might be due to any or all of a number of causes—electrical changes in the tissues produced by the passage of sound waves, electrical changes originating in the cochlea, or acoustic action currents. The occurrence of a microphonic effect under these conditions in no way proves, however, that the microphonic phenomena dependent upon vibration of the electrodes were not present in the other experiments and will not also appear when the conditions essential to them are provided—contact of the electrodes and the tissues.

(*5*) *Source of electric effects.* Assuming that all the microphonic phenomena previously considered have now been shown to be absent, the authors ask "whether the electrical effects shown to be correlated with the sound have their source in the sense organ or in the nerve." As conclusive evidence in favor of nerve transmission, the following experiment is reported: "The right cochlea was destroyed completely, and the impulses tested from both the left nerve and the brain stem. The left auditory nerve was then severed. The responses were no longer obtainable from the brain stem. They were still obtainable from the stump of the left nerve, however, and con-

[38] *Ibid.*, 385 f.

tinued to be so for some time. Just after severing the nerve the responses from the stump were of normal intensity, but the intensity died away during the next few minutes, and finally the signals ceased. Care was taken in the transection of the nerve to leave the tissues in contact, and a quantity of salt solution was introduced before and after the transection; it seems reasonable to say that the electrical conductivity of the tissues was not appreciably altered by severing the nerve. As the responses were completely eliminated, our conclusion is that these responses are due to nerve conduction (cat 11)."[39] The logic of the authors seems to be then that the transection of the nerve, especially since a quantity of salt solution was introduced, should not have produced any change in the electrical conductivity to tissues beyond the cut, while it would stop the propagation of nerve impulses beyond this point. That the effect was eliminated on the far side of the cut is taken to indicate that the effect could not have been due to electrical spread from the cochlea; that it was still found in the stump of the nerve is regarded as in agreement with what would be expected if the effect were due to action currents.

This argument is faulty for two reasons: first, because the electrical conductivity from the cochlea to the brain stem may not be considered as good after the section of the nerve as before; and secondly, because the electrical changes detected at the stump of the nerve could not have been due to action currents generated at that point. There are a number of reasons why it may not be assumed that the electrical conductivity from the cochlea to the brain stem was as good after the nerve was severed as before. Kato, in another connection, has shown that even crushing a nerve decreases its electrical conductivity.[40] Moreover, the nerve could scarcely have been severed without severing the auditory artery that runs along with it, as has been suggested too by Adrian. Loss of blood upon collapse of the vessel might be expected to alter the conductivity of the entire 'cable' composed of the auditory and facial nerves and auditory artery. Results from the therapeutic administration of electric currents to the body indicate that the currents, to a large extent, follow the paths laid out by the blood vessels. Therefore, cutting the blood vessel would very materially alter the conductivity along the path traced by the vessel. Moreover, the injection of salt

[39] *Ibid.*, 386 f.

[40] G. Kato, *The Theory of Decrementless Conduction in Narcotized Region of Nerve*, 1924, 75.

solution might be expected to hinder the transmission of electrical changes to tissues beyond the place of the cut, rather than to aid it. For a given quantity of electricity that was spreading out from the cochlea to regions of lower potential would be dissipated throughout the mass of the liquid injected. The amount of electricity that would reach the stem beyond the cut would be so much the smaller on account of much of the charge having been thus dissipated. For these reasons, it is more probable that the electrical conductivity between the cochlea and the brain stem was decreased by cutting the nerve than that it was not.

That the effect detected at the stump of the nerve was due to action currents is improbable. If the nerve had been cut it would be showing a steady injury potential at the stump. Action potentials could not be detected at that point since action potentials disappear when the nerve impulse reaches a region of injury, or rather, the nerve impulse as well as the action potentials are extinguished on reaching a region of injury. It is this very fact that is made use of in physiological technique for obtaining monophasic action currents instead of diphasic ones, the fact, namely, that an electrode resting upon an injured place is not affected by the action potentials of nerve impulses being transmitted toward that point. Consequently, any electrical changes picked up by the electrode at the stump of the nerve must have been conducted to it by the tissues at the stump acting as an inert conductor of electrical potentials generated elsewhere.

The results of this experiment may, however, be readily accounted for without the difficulties involved in the 'action current hypothesis' given by Wever and Bray. Cutting the left nerve probably also involved the rupture of the artery running along with it. This may have had as effects: a decreased conductivity of the tissues from the cochlea to the brain stem; a decrease in the 'strength' of functioning of the cochlea, due to interference with its blood supply (Adrian reported that a disturbance of the blood supply led to a decrease in the microphonic effects obtained from the cochlea); and finally a change in the electrical properties of the tissues about the electrodes (inter-electrode potential, and the capacity and resistance determining the impedance of the input circuit). The decrease in the 'strength' of functioning of the cochlea would lead in turn to a decrease in the amplitude of the electrical changes produced there, as well as to a decrease in the energy of sound vibrations in the cochlear liquids,

and therefore of the vibrations propagated from these liquids to the tissues outside. All these changes may have contributed to the loss of the microphonic effect on the stem. The fact that the effect was still obtainable from the stump might be accounted for by the facts that: (1) the electrode, being pressed up against the stump, and the petrous bone, would be in a better position to respond to any sound vibrations that were being propagated through the bony wall of the cochlea than when resting on the tissues of the brain stem; (2) the inter-electrode potential might be greater now, on account of one electrode resting on the freshly cut nerve; and (3) there would be a better electrical conducting path between the electrode and the source of electrical changes in the cochlea.

This last experiment completes the checks performed by Wever and Bray to demonstrate that microphonic phenomena other than action currents of the auditory nerve were not responsible for the bio-microphonic effect. Except for the measures taken to avoid the microphonic effect of the tubes, it is apparent, however, that the experiments performed have not been adequate to show that the other phenomena considered were not factors in the effect.

Adrian. The procedure followed by Adrian to determine whether the microphonic effect was due to action currents in the auditory nerve was somewhat different from that of Wever and Bray, who, as we have seen, chose as their point of attack certain of the phenomena which may have been responsible for the effect other than action currents. Adrian concerned himself, rather, with the influence upon the effect of a number of conditions that might be expected to eliminate or seriously interfere with action currents. As a result of these experiments, he concluded that "it is almost certain that the electrical changes are generated in the cochlea and are not due to nerve impulses. The chief reasons for this view are that the effects are obtained with electrodes so far from the nerve that the usual action potentials would have little chance of reaching them, and that they are unchanged by conditions which would seriously interfere with nerve conduction."[41] He states further that "the effect led off from the nerve, the medulla, or the cerebellum can scarcely be due to impulses in the nerve fibers under the electrodes since it persists unchanged after the bare nerve has been covered with novocaine crystals or with lumps of ice or injected with 5% acetic acid. It is not then due

[41]Adrian, *op. cit.*, xxviii-xxix.

to nervous conduction from the opposite cochlea, for this may be destroyed. Partial crushing of the nerve makes little or no difference. Crushing the entire nerve or cutting it through reduces or abolishes the effect, but the arteries to the inner ear run with the nerve and I have found it impossible to avoid damaging them as well. As Wever and Bray have shown, interference with the blood supply usually abolishes the effect, possibly owing to pressure changes in the inner ear." Adrian points out that the detection of the electric changes by an electrode on the nerve is understandable since the internal meatus is a hole "which will provide a low resistance pathway into the inner ear and an electrode on the nerve will tap this pathway." He considers it unlikely that the electric changes are generated by nervous elements in the cochlea since the reproduction of sound may persist unaltered after cooling the petrous bone with ice. He finds that "oscillograph records reproduce the wave form of the sound and the curves show no sign of being built up of nerve impulses." Adrian's stand is then that the electrical changes detected by him were probably generated in the cochlea, that it is improbable that these were produced by nerve elements in the cochlea, and that the effect which was detected by an electrode on the nerve was merely due to the nerve conducting the electrical changes as would an inert conductor, and was not produced by action currents of the nerve.

Adrian, Bronk, and Phillips. The experiments of Adrian, Bronk, and Phillips were performed to determine to what is due the electrical changes that can be detected in the cochlea.[42] As a result of the experiments, they state that the objections previously raised by Adrian to Wever and Bray's explanation of the microphonic effect turn out to be not very serious, and conclude that the electrical changes in the cochlea are due to nervous structures of some sort. They add that the situation of the nerve fibers in the cochlea may favor the development of large potentials and that the intra-cranial part of the nerve may appear to act as an inert conductor because it is unfavorably placed for electrical recording and more liable to damage than the intra-cochlear part. It should be noted, however, that the experiments of Adrian, Bronk, and Phillips do not change at all the validity of the results of Adrian's previous experiments which tend to show that the electrical changes led off from *the nerve* were not due to impulses in the nerve fibers under the electrode. For the present

[42]Adrian, Bronk, and Phillips, *op. cit.*, 2P-3P.

investigation is not upon this point; it is concerned only with the conditions of the effect which can be picked up by an electrode on the wall of the cochlea.[43]

We may now examine the experiments which led these authors to the conclusion that the electrical changes picked up from the cochlea were due to nerve elements of some sort in the cochlea. To support this conclusion, three lines of evidence are presented: (*a*) final disappearance of the microphonic effect after death of the animal or after failure of the blood supply to the inner ear (after 1½ hr. for the guinea pig and about ½ hr. for the cat); (*b*) failure of the effect as a result of injections of cocaine solution through the round window or the wall of the guinea pig's cochlea; and (*c*) weakening of the effect upon cooling the cochlea of the guinea pig with ice. The authors conclude that "these results all indicate that the Wever and Bray effect is due to living cells and cease with their death, and the action of cocaine makes it likely that nerve endings, nerve cells or nerve fibers are responsible."[44]

Let us consider first the evidence that the microphonic effect is due to living cells. In order for the evidence to be conclusive there must be first a precise correlation between the changes that the experiments would be known to produce in living cells and the changes occurring in the microphonic effect; and secondly, the changes produced in the effect should be explainable in no other way.

(*a*) *Death or failure of blood supply.* In the experiments upon the effect of death or failure of the blood supply to the inner ear, there was found to be an initial fall in intensity when the circulation failed, then a continuation of the effect on about the same intensive level for from 1-1½ hr., and finally a complete disappearance of the effect. The authors attribute the initial fall in intensity to a mechanical cause such as the collapse of vessels, and not to a failure of the oxygen supply. The fact that the effect finally disappears is taken to indicate that the electrical changes are due to the activity of living cells. The necessary positive correlation of changes in the effect and in living cells is then not very complete. For there is no evidence that living cells did or would be likely to cease functioning at the particular time that the microphonic effect failed. All that can be said is that this may have occurred. Furthermore, the final failure of the effect may have been due to other causes, *e.g.* the final drying up of the liquids beyond a certain necessary level.

(*b*) *Cocaine injections.* A rapid failure of the effect was produced by injection of 10–20% cocaine solution into the round window or through the walls of the

[43]In this experiment, the electrical changes from the cochlea were picked up by a moist thread electrode placed on the ventral surface of the petrous bone and it was therefore unnecessary to open the cranium, as when the auditory nerve is exposed.

[44]Adrian, Bronk, and Phillips, *op. cit.*, 3P.

cochlea. "The injection of saline," the authors state, "usually leaves the effect unimpaired if no great damage is done," and add that "although the variable amount of damage confuses the issue the effect of cocaine seems incontestable."[45] It is difficult to regard this experiment as conclusively demonstrating the influence of cocaine, in view of the lack of precise knowledge of the damage done inside the cochlea by the various injections. That the cocaine was the responsible agent could hardly be considered conclusive without statistical data. But even though the cocaine should be regarded as a contributing factor in the failure of the effect, it need not be concluded that living cells were therefore involved. Such a conclusion would be justified only if the influence of cocaine could be regarded as limited to living cells. A significant property of cocaine, as well as of other narcotics, is that it is a non-electrolyte; and its influence upon living cells probably depends in large measure upon this property (in conjunction with other essential properties).[46] But the production of electrical fluctuations in the cochlea and their conduction to the electrodes must depend in some way upon the presence of electrolytes, upon the possibility of changes in the concentration of ions. Injection of a non-electrolyte would naturally produce changes in the concentration and state of equilibrium of any electrolytes that might be present in the cochlear liquids. There seems no need for assuming that these changes must take place inside of living cells.

(*c*) *Cooling the cochlea.* The weakening of the effect upon cooling the cochlea does not necessarily indicate the participation of living cells. Temperature is a noteworthy condition of many physical and physico-chemical processes not involving life, including many of the phenomena involved in the generation of electromotive force in electrolytes.[47] The effect of temperature upon such processes might have been responsible for the weakening of the microphonic effect obtained from the cochlea.

The evidence that the living cells, if they are actually involved, are *nerve cells* is still less decisive. The only evidence presented is the result of the experiments with cocaine, the inconclusiveness of which has already been indicated. Besides, the action of narcotics is not limited to nerve cells; they interfere as well with the functioning of other kinds of living cells.[48]

As for the significance of the conclusion that nerve elements of some sort are involved in the production of the electrical changes in the cochlea, even though this statement had been conclusively established, it would not follow that such electrical effects are likewise generated along the extent of the auditory nerve. The electrical changes in the cochlea might have arisen at the surfaces of contact between the nerve-endings and the liquids of the cochlea. It is well

[45] *Ibid.*, 3P.

[46] See discussion on theory of counter forces and capacity in Gildermeister, *op. cit.*, 690-693; 688.

[47] F. H. Getman, *Outlines of Theoretical Chemistry*, 1926, 4th ed., 527-618.

[48] W. M. Bayliss, *Principles of General Physiology*, 1924, 4th ed., 140 f.

known that the surfaces of contact between two different media have properties very different from the main body of either medium alone.[49] Therefore, even if electrical changes of the frequency of the stimuli should be generated at the junctions of the nerve-endings and the cochlear liquids, it could not be assumed that like changes would occur throughout the extent of the auditory nerve, as action currents of impulses in this nerve. (Though, of course, one should expect such electrical changes in the cochlea to be conducted along the nerve in its rôle as an inert conductor.) Finally, it may be pointed out that the experiments of Adrian, Bronk, and Phillips do not demonstrate that the electrical effects detected were actually generated within the cochlea itself. They may also have been generated at the electrode by the sound waves transmitted through the walls of the cochlea.

Wever. In this study, Wever reports the detection of the 'bio-microphonic effect' in the guinea pig, rabbit, and rat.[50] He reports one new check, which was designed to test the hypothesis that the bio-microphonic effect "might be due not to nerve impulses but to a condenser action of the drum and associated ossicles."[51] The check was performed on one guinea pig and on one cat. It consisted in the destruction of the right cochlea and of the left vestibule, care being taken to avoid modification of the ear drum and the ossicles. Upon the completion of these operations the microphonic effect failed to occur. This check, though it may exclude the hypothesis it was designed to test as a possible explanation of the microphonic effect, obviously does not exclude as possible causes any of the microphonic phenomena dependent on the occurrence of sound waves in the cochlea or other parts of the labyrinth, since they would all be eliminated by the operations performed.

Davis and Saul. The experiments of Davis and Saul involved certain differences in experimental technique from that of their predecessors.[52] A stethoscope was fitted to a decerebrate cat. Sound stimuli could be sent to one ear or the other by clamping the appropriate arm of the stethoscope. The stimuli consisted of taps delivered to the bell of the stethoscope and of other sounds delivered through a funnel in place of the stethoscope bell. The electrodes consisted of a diffuse silver electrode placed subcutaneously on the muscles of the

[49] *Ibid.*, 71.
[50] Wever, *op. cit.*, this JOURNAL, 457-462.
[51] *Ibid.*, 459.
[52] Davis and Saul, *op. cit.*, 205 f.

neck and a fine silver needle electrode, insulated except at the tip, which was placed on the tissues where the electrical response was to be tested. The recording instrument consisted of one stage of amplification with string galvanometer or of more stages (number not stated) with telephone. The investigators were especially interested in determining the nature of the responses that could be detected in the brain stem, by pushing the needle electrode caudally down the stem, at right angles to the plane of decerebration.

The authors concluded from their results that the microphonic effects they obtained in response to acoustic stimuli were due to true action currents in the auditory pathways. These results were as follows: (*a*) the responses were much greater in the auditory pathways than in surrounding tissue; (*b*) there was immediate cessation of the response upon the death of the animal; (*c*) there was a decrease in the size of the response upon the local application of narcotics. The authors believe that the brain stem effect involved two factors: first, action currents in the particular auditory tract to which the electrode is applied; and secondly, electrical spread from distant parts of the auditory mechanism.

Let us consider the experimental evidence. Here too, in order for the results to be regarded as showing conclusively that auditory action currents were involved in the microphonic effect, it is necessary first, that there be a precise correlation between the changes in the effect due to a given condition, and the probable change in the functioning of the auditory nerve under these conditions; and secondly, that the changes in the effect be explainable in no other way.

(*a*) *Limitation of the maximum effect to the acoustic pathways.* Responses in the brain stem were found to be sharply localized. "Using the string galvanometer with one stage of amplification, responses were obtained," Davis and Saul write, "only from certain points. A movement of the electrode of 1 mm. or less suffices to pass through a maximum and usually to lose the response. With more sensitive amplifier and ear phones, this point represents a very sharp maximum many times louder than the responses from neighboring points. These active points have been shown by subsequent gross section to lie invariably upon the auditory pathways. . . . When electrodes are placed upon crossed tracts, such as the lateral lemniscus, the responses are chiefly contralateral, *i.e.* the electrode in the left midbrain is much more sensitive to stimuli applied to the right ear than to those applied to the left ear."[53] These results agree very closely with what would be expected if the microphonic effect were due to auditory action currents, and to that extent support the action-current hypothesis. They do not, however, exclude all other possible hypotheses. There is no certainty that sound waves

[53] *Ibid.*, 205.

and electrical changes propagated from the cochlea would not also find a better conducting path along the acoustic pathways. As for electrical conductivity, it was early found by Hermann that the conductivity of nerves was five times better in a lengthwise direction than in a crosswise direction.[54] That the acoustic pathways may represent pathways of higher electrical conductivity thus has some experimental support. We have mentioned above some of the reasons for supposing that the sound waves propagated from the cochlea might follow with less opposition the pathway laid out by the nerve. It is conceivable, therefore, that the microphonic effects obtained by Davis and Saul in these experiments may have been due to vibration of the active electrode produced by sound waves, potential changes generated in the tissues by the passage of sound waves, and electrical spread along the nerve from the cochlea, as well as to action currents in the acoustic pathways. Before the latter hypothesis can be regarded as necessarily true, the others must first be excluded as possibilities.

(*b*) *Death of the animal.* "When the cat dies in the course of an experiment, the responses from the brain stem are lost," Davis and Saul write, "as soon as the heart stops beating, and sometimes a few minutes before. The responses from the acoustic nerve persist after those from the brain stem are no longer detectable, and sometimes for several minutes after the heart has stopped beating."[55] This result is not in complete agreement with that found by Adrian, Bronk, and Phillips for the microphonic effects obtained from the cochlea. They found that there was an initial decrease in the intensity of the effect after death, but that the effect continued at a constant level of intensity for from 1–1½ hr. longer in the guinea pig and about ½ hr. in the cat, before finally disappearing.[56] Wever and Bray found that the effect picked up by an electrode on the nerve lasted about 20 min. after the death of the cat. Adrian, Bronk, and Phillips interpreted the initial decrease as due to a collapse of vessels of some sort, and the final disappearance of the effect as due to the death of nerve cells or fibers, which they believed were responsible for the microphonic effect.[57] If Davis and Saul wish to use the result they found as an indication that the effect was due to acoustic action currents, they must assume that loss of function of the acoustic pathways occurred immediately with death, with the failure of the circulation. If might possibly be claimed, in order to reconcile the two sets of results, that nerve cells in the brain and the nerve fibers leaving the inner auditory meatus cease functioning immediately after death (in accordance with the results of Davis and Saul) and that the nerve elements in the cochlea continue functioning for about ½ hr. after death (in accordance with the interpretation of Adrian, Bronk, and Phillips). But however one may seek to reconcile these results, it is apparent that the changes in the microphonic effect with death cannot be considered as supplying evidence that the effect is due to auditory action currents until there is more precise knowledge of just what is the effect of death upon the functioning of the auditory nerve and pathways, and how soon after death functioning there ceases. Without this information, a correlation of nerve functioning and the microphonic effect cannot be alleged. Furthermore, even on the hypothe-

[54] Hermann, *Lehrbuch der Physiologie*, 239.
[55] Davis and Saul, *op. cit.*, 205.
[56] Adrian, Bronk, and Phillips, *op. cit.*, 2P.
[57] *Ibid.*, 3P.

sis that the microphonic effect was due to some one or a number of the microphonic phenomena *not* involving action currents, a decrease or elimination of the effect with death might be expected. For as noted above, death might lead to an alteration in a number of the factors on which these other microphonic phenomena depend: *e.g.* decreased 'strength' of functioning of the cochlea with consequent decrease in the magnitude of the mechanical and electrical fluctuations propagated from the cochlea; increase in electrical impedance of the tissues between the active electrode and the cochlea; and changes in the injury potential difference, capacity, and resistance of the tissues.

(*c*) *Effect of narcotics.* "Narcotization of parts of the brain stem by injecting 5 or 10% novocaine in Ringer's solution near the electrode obliterates responses on the string. The responses return after 1–3 hr. If the novocaine is injected on one side of the brain stem between the electrode and acoustic nerve, responses from that ear disappear, while those from the other ear persist."[58] These results too disagree with those found by Adrian and by Wever and Bray.[59] These investigators found that the microphonic effect obtained with an electrode on the auditory nerve at its exit from the inner auditory meatus persisted despite the application of narcotics to the nerve. Such differences in results may be due to differences in the technical conditions of the various experiments; but for the results with narcotics to have a clean-cut significance such disagreements must be reconciled. But in any case, the elimination of the microphonic effect by cocaine would not compel one to attribute the effect to neural action currents. Injection of cocaine in the tissues near the active electrode might be expected to alter certain essential electrical properties of the tissues in this region.[60] As a result of changes in the capacity and resistance of the tissues, in the inter-electrode potential, and in the electrical impedance of tissues between cochlea and electrode, there might have occurred a loss of microphonic effects due to vibration of the electrode, to the propagation of electrical changes from the cochlea, or to the generation of electrical changes in the tissues by the passage of sound waves. Further, where cocaine is injected between the electrode and the auditory nerve of one side, the microphonic phenomena that might be expected to be altered would depend upon the extent to which diffusion of the narcotic could affect the tissues surrounding the active electrode. If the narcotic diffused so far, the microphonic phenomena which might have been eliminated by the narcotic would be the same as those itemized above; the persistence of the effect from the opposite ear might indicate merely that the average magnitude of the changes in the tissues on the side where the narcotic was injected were greater than on the other side—that here the changes in the electrical properties of the tissues were not great enough to eliminate the effect. So the results with cocaine cannot be considered conclusive until the disagreements of the different investigators have

[58] Davis and Saul, *op. cit.*, 205.

[59] Adrian, *op. cit.*, xxix; Wever and Bray, Action currents in the auditory nerve in response to acoustic stimulation, *Proc. Nat. Acad. Sciences*, 16, 1930, 348.

[60] Gildermeister, *op. cit.*, 678, 688; W. J. V. Osterhout, *Injury, Recovery and Death, in relation to Conductivity and Permeability*, 1922, 184-194; Crile, Hosmer, and Rowland, *op. cit.*, 98; G. W. Crile, H. F. Rowland, and G. H. Crile, The electrical capacity of animal tissues under normal and pathological conditions, *Amer. J. Physiol.*, 76, 1926, 323.

been reconciled and the exact influence which the cocaine had on the properties of the tissues essential to the other microphonic phenomena is known.

Hughson and Crowe. The purpose of this investigation was not to determine the nature of the phenomena responsible for the 'bio-microphonic effect,' but rather to use this effect as an indicator of the influence of various parts of the middle ear on the transmission of sound vibrations to the cochlear liquids.[61] The extent to which this procedure seems justifiable will later be examined; but now we wish to determine whether the results of these authors provide proof of the neural origin of the effect. The experimental procedure consisted in placing the active electrode on or near the auditory nerve (of the cat) and the indifferent, grounded electrode in the muscles of the neck. Wires connected the electrodes to a 6 tube amplifier (with telephone or loud speaker) in another part of the building. The experimental operations performed were of three kinds: (a) experimental lesions that produced *no appreciable change* in the effect—such as puncture of the ear drum, and filling the niche of the round window with wax or plaster; (b) lesions that produce a *decrease* in the effect—such as fixation of the ossicular chain, division of one of the ossicular joints, interference with the movements of the malleus, and puncture of the membrane of the round window (puncture of the round window membrane produced a great decrease in the intensity of the effect, the operations upon the ossicular chain affected especially the lower frequencies); (c) experimental lesions that produce an *increase* in the intensity of the effect, "increase the volume and clarity of the spoken voice and pure tones,"—such as an increase in pressure on the round window membrane.

(*a*) *No change.* The experiments where no change was produced in the microphonic effect obviously supply no evidence supporting the action current hypothesis. If anything, they furnish contrary evidence. Whatever reasons there are for expecting puncture of the drum or blocking of the round window to decrease sensitivity to sound would also lead one to expect a corresponding change in neural functioning and, therefore, in the microphonic effect, if it were due to action currents. The results might be interpreted, however, as showing merely that the microphonic effect is not sensitive enough to indicate changes in the neural response of the magnitude that would be produced by these lesions.

(*b*) *Decrease.* Consider now the experiments where there was a decrease in the effect. Various operations upon the ossicles led to a decrease in the microphonic effect. Consequently, there must have been at least a fraction of the original effect which was dependent upon proper functioning of the ossicular chain. This decrease in the effect might very readily be explained by the action current

[61]Hughson and Crowe, *op. cit.*, 2027-2028.

hypothesis. For such operations upon the bone chain might be expected to lead to a decrease in the energy of sound vibrations transmitted to the cochlea, a decreased intensity of stimulation of the nerve endings in the cochlea, and therefore decreased intensity of auditory nerve response and of the accompanying action currents. A decrease in the energy of the sound waves in the cochlea would, however, also decrease the energy of the vibrations propagated through the cochlear walls to the electrode and so might be expected to produce a decrease in the microphonic effects dependent on such transmission of sound vibrations. Furthermore, if the magnitude of the *electrical changes* generated in the cochlea depended on the energy of the sound waves in the cochlea, they too would be decreased or eliminated when the sound waves transmitted to the cochlea decreased in energy. It should be noted that the effect was not eliminated, but merely decreased in intensity by operations upon the ossicular chain. Which of the various microphonic phenomena contributed to this residual effect there is no way of deciding from the experiments. Any of the phenomena may have been involved, those not dependent upon the occurrence of vibrations in the cochlea as well as those dependent upon this source. The decrease in the microphonic effect produced by puncture of the round window membrane has the same significance as the decrease produced by the various operations on the ossicles. Such a puncture might be expected to lead to a decrease or disappearance of the microphonic effect due to *any* of the phenomena dependent on the energy of the sound waves occurring in the cochlea. Therefore the decrease that was found may have been due to a change in any or all of these microphonic phenomena, and there is no special evidence in favor of the action current hypothesis. The residual effect here, as in the case of that remaining after operations on the ossicular chain, may have been due to all or any of the various microphonic phenomena.

(*c*) *Increase.* Let us consider finally the experiments in which an *increase* in the intensity of the microphonic effect was produced by an increase in the pressure on the round window membrane. There is no way of deciding the question: which of the various conceivable microphonic phenomena contributed to the effect before the increase in pressure? All or any of them may have been involved. The only part of the total effect that permits more specific conclusions is that representing the increase, the increment in the effect. This increase in the effect indicates that the increased pressure made more favorable some factor upon which at least a fraction of the total effect now depends. The question at issue here is whether the increment in the microphonic effect would be expected if the effect were due to action currents of the auditory nerve. That is to say, would pressure on the round window membrane be expected to lead to an increased magnitude of response of the auditory nerve? As Hughson and Crowe state, it has generally been believed that an increase in the intra-labyrinth pressure *interferes* with normal hearing. One should expect then that the increased pressure would be correlated with a decrease in the intensity of the neural response, and consequently, a decrease in the intensity of the microphonic effect if it were due to action currents of the nerve. The opposite result was found; this result, then, not only does not support the action current hypothesis; it is in opposition to it.

Hughson and Crowe suggest, despite the indications of other lines of evidence, that the increased pressure on the round window membrane may effect an increase in the "transmission of sounds" through the ear. If such an increase

occurs, it might be expected, however, to lead to an increase in the intensity of the effect due to all or any of the microphonic phenomena dependent upon the occurrence of sound vibrations in the cochlea. So even this supposition does not lend any advantage to the hypothesis that the effect is due to action currents in the auditory nerve. We may conclude then that the experiments of Hughson and Crowe fail, as do the other investigations, to furnish the conclusive evidence in favor of the action current hypothesis which we have been seeking.

A few comments may now be added upon the attempt of Hughson and Crowe to use the *bio-microphonic effect* as an indicator of the functions of various parts of the middle ear in the transmission of sound to the inner ear. It is apparent that before the effect can be accepted as a reliable indicator of such functions, it must first be shown that there is in fact a precise correlation between the energy of the sounds transmitted to the cochlea and the magnitude of the microphonic effect. This correlation has not yet been demonstrated; consequently any conclusions upon the functions of various mechanisms in the ear, made on the basis of the microphonic effect, must be regarded as tentative only. One may, however, examine the experiments of Hughson and Crowe to see whether these supply evidence that the correlation mentioned does exist. Upon examination, the results turn out to be contradictory in nature. Puncture of the ear drum might be expected to decrease somewhat the energy of the sound vibrations transmitted to the inner ear, but no change occurs in the microphonic effect. Various operations on the ossicular chain, which might be expected to decrease the energy of the vibrations transmitted, do in fact lead to a decrease in the microphonic effect. A similar agreement occurs in the case of experiments involving the puncture of the round window membrane. Blocking the niche of the round window or increased pressure upon the membrane, either of which procedures might be expected to lead to a decrease in the energy of the sound vibrations in the cochlea, leads to no such corresponding changes in the microphonic effect. Blocking leads to no change at all, and increased pressure on the membrane leads to an increase in the effect. It is clear then that the results of this investigation as a whole may hardly be considered as furnishing support for the thesis that the correlation mentioned exists; some of the results support this thesis, other results do not, and still others may be considered contradictory to it.

Hughson and Crowe, in the paper under discussion, were primarily concerned with determining the function of the round window membrane. On the basis of the increase in the microphonic effect which was produced by pressure on the

membrane they concluded: "We are convinced therefore that the round window membrane acts as a safety valve for the inner ear structures and by its mobility effects approximately a 50% absorption of all sound entering the ear. If the membrane is made rigid by pressing on it with a plug of moist cotton, the perception of spoken words and practically all tones is increased at least 50%."[62] But even if the bio-microphonic effect detected could be accepted as a reliable indicator of the energy of the sound vibrations in the cochlea, the results hardly seem to warrant the conclusion that the function of the round window membrane is to *act as a safety valve* to protect inner ear structures. For the function of a safety valve, which is incorporated in a physical system of some sort, *e.g.* a pressure system, is to allow the escape of energy *only when it exceeds* a certain dangerous level, and not at levels beneath this. A device which released energy beneath the level at which it became dangerous would not be helping in any way to protect the structures within the system, but merely serving to render the system less efficient. Such a device might better be called an attenuator than a safety valve. So even if it were true that the mobility of the round window membrane is responsible for a "50% absorption of all sound entering the ear," that is to say, if it were responsible for a reduction in the energy of *all vibrations* occurring in the cochlea, this fact would not justify the hypothesis that the function of the membrane is that of a safety valve.

Finally, we may briefly consider some of the reasons which make it improbable that increased pressure on the round window membrane does lead to an increase in the energy of the sound vibrations occurring in the cochlea, despite the reported increase in the intensity of the microphonic effect. Certain physical grounds indicate that mobility of the round window membrane serves to *increase* the energy of the vibrations that may occur in the cochlea rather than decrease it. Essentially, the cochlea is like a tube of water closed at one end by an elastic membrane (the round window membrane) and at the other by a piston which can move back and forth (the base of the stapes) and containing within the tube an elastic membrane (basilar membrane) which can be set into motion by the movements of the liquid.[63] It seems clear that if the membrane at the end of the tube is made rigid, the piston will not be able to set the liquids in the tube into motion (since the liquid is practically incompressible), unless some other part of the walls of the tube can move under the changing pressures produced by the piston. Increasing rigidity or tension of the membrane should lead then to a *decrease* in the energy of movement of the

[62] *Ibid.*, 2028.

[63] For a discussion of recent ideas on the dynamics of the inner ear, see H. Fletcher, *Speech and Hearing*, 1929, 111-125; R. L. Wegel and C. E. Lane, The auditory masking of one pure tone by another and its probable relation to the dynamics of the inner ear, *Phys. Rev.*, 23, 1924, 276-285; G. v. Békésy, Sur la théorie de l'audition, *Année Psychol.*, 31, 1930, 63-96.

liquid in the tube. Moreover, if the walls of the tube are capable of responding at all to the changing pressures produced by the piston, then with increasing rigidity of the end-membrane of the tube, relatively more and more energy will be transmitted through the walls. That is to say, as one pathway becomes blocked, relatively more energy will flow through other pathways; just as in an electrical system, when the resistance of one of a number of parallel pathways increases, a greater proportion of the total current flows through the other paths. And such a phenomenon may account for the increase in the intensity of the microphonic effect reported by Hughson and Crowe. A larger proportion of the energy of the vibrations produced at the oval window might now be transmitted through the walls of the cochlea, and through the liquids of the vestibule and semicircular canals and their enclosing walls, since the path by way of the round window was blocked. More sound energy might thus be transmitted through the bony walls of the labyrinth even though the energy of the vibrations occurring in the liquids of the cochlea might have decreased. A corresponding increase in the intensity of the microphonic effects due to the vibration of the electrode and the electrical changes generated in the tissues by the sound waves might then be expected. An explanation of this sort makes unnecessary a conflict with clinical and physical evidence such as occurs on the assumption that increased pressure on the round window membrane leads to an increase in the energy of sound waves in the cochlea.

Rademaker and Bergansius. Rademaker and Bergansius were concerned with registering the bio-microphonic effect with string galvanometer and determining how it varied with different frequencies and intensities of stimulus, rather than with examining the conditions upon which the effect depended.[64] The curves of the electrical changes produced by tonal stimuli (tuning forks, organ pipes, and voice) corresponded in all particulars with the galvanometric curves of the stimulating sound waves. The curves of the microphonic effect obtained from simple tonal stimuli were sinusoidal in form, as closely as can be judged from the reproductions of the curves; the curves of the electrical changes produced by a sung vowel showed characteristic higher partials along with the fundamental. Increases in intensity of tonal stimuli led to an increase in the amplitude of the curves of the microphonic effect.

[64]Rademaker and Bergansius, *op. cit.*, 346-349.

Although Rademaker and Bergansius performed no experiments upon the conditions of the microphonic effect, the results which they obtained upon the form of the curves may throw some light on whether or not the effect is due to action currents in the auditory nerve. Before going on to discuss the significance of these results, we may note that they agree with what has been said by certain of the previous investigators concerning the form of the electrical changes constituting the bio-microphonic effect. In a number of places Wever and Bray indicate that the electrical effects obtained by them from simple stimuli were sinusoidal in form. In one place, they say, "a pure tone sounded into the ear of the cat is recognized as that same tone in the receiver."[65] In a later note, this sine-wave character of their microphonic effect is indicated in a statement made concerning the freedom of their apparatus from distortion effects: "our amplifier and telephone receiver system will not distort to the extent of giving a sine wave type of response to a signal that departs at all widely from it" and add later that work with a cathode ray oscillograph confirmed the indications of the telephone receiver concerning the form of the electrical response.[66] Adrian too, it will be recalled, said that "oscillograph records reproduce the wave-form of the sound and the curves show no sign of being built up of nerve impulses."[67]

These results acquire some significance in indicating whether the microphonic effects obtained were due to action currents when we consider certain facts relating to the transmission of sound waves to the cochlea by the chain of ossicles in the middle ear. As is well known, vibrations transmitted to the inner ear by the ossicles undergo the effect of so-called non-linear distortion. That is to say, equal changes in pressure on the ear drum do not result in equal displacements of the foot of the stapes at the oval window. The result is that sound waves impressed on the ear drum are distorted in their transmission to the cochlea, in such a fashion that a sine-wave stimulus will result in a wave containing not only the impressed frequency, but a series of its harmonics as well. As the intensity of the stimulating sound increases, the amplitude of the harmonics *relative* to the fundamental will likewise increase.[68] If the action current hypothesis is correct, that is to say, if the frequencies occurring

[65] Wever and Bray, *op. cit.*, *J. Exper. Psychol.*, 377.
[66] E. G. Wever and C. W. Bray, A note on "A neglected possibility in frequency theories of hearing," this JOURNAL, 44, 1932, 193.
[67] Adrian, *op. cit.*, xxix.
[68] Fletcher, *op. cit.*, 124, 312 f.; L. T. Troland, *Psychophysiology*, II, 1930, 246 f.

in the bio-microphonic effect correspond to the frequencies present in the response of the auditory nerve, and if these in turn correspond to the frequency of the vibrations in the cochlea—then the harmonic frequencies introduced by the ossicles in the transmission of sound to the cochlea should be represented in the microphonic effect as well as the frequencies present in the original physical stimuli. The results of the various investigations which have shown that the microphonic effect obtained from simple stimuli are sinusoidal in form fail to confirm this consequence of the action current hypothesis and so indicate that the electrical changes detected were not due to auditory action currents.

It is desirable, however, that a more precise test of this point be made than is provided by the data so far presented. That is to say, what is required is oscillographic records (obtained with a high frequency oscillograph) of the bio-microphonic effect obtained from simple tonal stimuli which range from stimuli of low intensity to those of intensities high enough to produce harmonics of appreciable magnitude. As the intensity of the stimulus increases, harmonics should appear in the curves of the microphonic effect and should increase in magnitude relative to the fundamental component, which corresponds to the stimulating frequency. The failure of this result to appear must then be regarded as a failure to confirm the hypothesis that the microphonic effect is due to action currents of the auditory nerve, and as an indication of the incorrectness of this view.[69]

Kreezer and Darge. When the experimental conditions described by Wever and Bray were duplicated by Kreezer and Darge, a microphonic effect was obtained.[70] This effect did not appear, however, when the precautions mentioned above in this paper (p. 642 f.) were taken to exclude the possibility of induction effects. The authors concluded, therefore, that the effect which they had obtained was due to induction from outside microphonic sources that were set into vibration by the sound waves used as stimuli.

Summary

Our survey is now at an end. Among the various investigations we have found a certain variety in the problems attacked and many

[69]But the converse of this is not true, that is to say, detection of these harmonics in the curves of the bio-microphonic effect will not prove that the effect is due to action currents, since the harmonics might appear as a result of any one of the microphonic phenomena dependent on sound waves in the cochlea, though not necessarily in the same degree.

[70]Kreezer and Darge, *op. cit.*, 105.

differences in the particular conditions of the 'bio-microphonic effect' which were investigated. In most cases in which the influence of the same conditions was examined by different investigators marked differences occurred in the results. Our search, furthermore, failed to reveal in any of the investigations a decisive experimental proof that the microphonic effect is due to action currents in the auditory nerve.

In certain of the investigations, the possibility was examined that certain phenomena *other than* acoustic action currents were responsible for the microphonic effect. Wever and Bray, who followed this procedure, concluded that the effect was *not* due to any of the other microphonic phenomena which they considered and that, therefore, it must have been due to auditory action currents, as the only remaining possibility. We found, however, that this conclusion was not justified, since the other microphonic phenomena were not conclusively eliminated as possible factors in the effect. Kreezer and Darge, who followed a similar procedure, concluded that the bio-microphonic effect which they obtained was due to induction, for it disappeared when adequate precautions for excluding induction effects were taken.

In other investigations, the procedure followed was to determine the influence on the microphonic effect of conditions that would be expected to influence it if it were due to auditory action currents. Adrian, whose experiments were of this sort, presented evidence opposing the 'action current hypothesis.' Adrian, Bronk, and Phillips went on to investigate the conditions of the microphonic effect which they could detect in the cochlea; theirs was *not* an investigation of the effect obtainable from the nerve. Their conclusion that the effect which they obtained was probably due to nerve elements of some sort in the cochlea is compatible with the evidence which they present but is not conclusively demonstrated by it. Davis and Saul investigated some of the conditions of the microphonic effect which they detected in the midbrain and in the auditory nerve. Their conclusion that it was due to action currents in the nerve is compatible with their results, but is not conclusively demonstrated by them, for certain other possible explanations were not excluded.

In still other investigations (Hughson and Crowe, Rademaker and Bergansius), the purpose of the authors was not to investigate whether the microphonic effect was due to auditory action currents; they were concerned, instead, with other problems. These studies were, however, examined but also found to be wanting in evidence capable of

demonstrating the correctness of the 'action current hypothesis.' On the contrary, a considerable part of the results of these investigations tends rather to refute the hypothesis that the microphonic effects obtained were due to action currents in the auditory nerve.

The general result of our survey is then that the evidence thus far presented fails to demonstrate that the microphonic effect is due to action currents of the auditory nerve. Hence, it is not possible as yet to conclude, on the basis of this evidence, that the frequencies of auditory stimuli are retained in the response of the nerve. Such a conclusion is premature; the question remains open. Whether it can be readily settled by means of the action current technique will depend on whether more decisive experiments can be devised and upon how readily the gaps in our knowledge, which prevent a precise interpretation of some of the experiments already made, may be filled in.

ELECTROPHYSIOLOGY OF MENTAL ACTIVITIES

By Edmund Jacobson, University of Chicago

The road to a science of psychology evidently must follow a rigorous examination of the question, What occurs in the organism simultaneous with and indispensable to mental activities? In the years 1829 to 1834, Weber made the studies on cutaneous and kinesthetic sensations which frequently are regarded as marking the beginning of experimental psychology.[1] After adding to these observations, Fechner,[2] in 1860, sought to make of psychology an exact science by developing the "fundamental relation or relations of dependency between the body and mind." This science, as is well-known, he called "Psychophysics," and its fundamental formula was, in effect, that sensation is proportional to the logarithm of stimulus. We need not here review the criticisms aroused by Fechner's interpretation; to find a scale to measure the experiences called sensations in terms of the objects or energies outside the body called stimuli has generally been considered impossible. According to the present investigation, in contrast with the views of Fechner, the goal should be the determination and measurement of nervous and muscular changes specific to the mental activity studied. This may reasonably be expected to open the road to find quantitative relationships between the nervous and muscular changes and their appropriate stimuli.

Classical psychologists, including experimentalists, at least until recent years, have employed some form of examination by the individual of his own activities. This approach was developed, chiefly by Wundt and his many disciples, with the aid of instruments and various mechanical devices, to permit repetition of observations under standard experimental conditions.

Wundt's attempts to link mental events to their physiological correlates,[3] particularly feelings to pulse and respiration, met with no startling success. Looking backward, we can see that he approached the problem primarily as a

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[1]E. Weber, *De Tactu*, in *Annotationes anat. et phys., programma collecta*, 1851; *Die Lehre vom Tastsinne und Gemeingefühle*, 1851.

[2]G. Fechner, *Elemente der Psychophysik*, 1889.

[3]W. Wundt, *Grundzüge der physiologischen Psychologie*, 5th ed., 1902.

systematic theorist. In spite of the vast array of technical methods which he employed, the influence of philosophical training on the psychologist is apparent. More recently, subsequent to experiments by Binet and various members of the Würzburg school, introspective methods were applied to the so-called higher intellectual activities; but here instruments seemed of little aid, and while the observations proved of great interest to some psychologists, to others—particularly Wundt himself—the methods appeared so crude as to be utterly unscientific. We were confronted in the days of Titchener with controversies, frequently acrimonious, resulting in divisions into schools of belief and marked by the absence of an incisive method to determine which opinion was correct. The poverty of human psychology at this stage as compared with other biological sciences was apparent.

Introspective psychologists have contended that only the individual is in a position to give adequate descriptions of his conscious activities as he himself experiences them. Accordingly the custom has developed of employing a number of self-observers during an experimental investigation and securing reports independently from each observer. Where such reports have agreed, conclusions have been drawn bearing on the character of conscious experience. But experimenters who have employed these methods have themselves called attention to what is, from the present point of view, the limitation of introspective psychology. They have pointed out that the introspector is called upon only to give accounts of his experiences as such; he should not report on his physiological processes. If he does, he commits the 'stimulus error.' Accordingly, we shall not expect to learn from the methods of introspective psychology what in the organism is indispensable to mental activities.

Nevertheless, methods of autosensory observation form the starting-point for the present studies. When we recall the delicacy of the human senses—their use, for example, in certain chemical tests to detect as little as one part of a substance diluted a billion times—we should expect them to be of service if we are ever to solve problems of the mechanism of conscious activities. What is required is that the reports of the senses, used directly in observation, be checked by objective methods, such as electrical measurement.

Despairing that psychology, based upon introspective methods, could ever become a real science, certain students under the leadership of Watson decided to limit their studies to problems of behavior. Some of them, in their enthusiasm over this limitation, proceeded to question the validity of all observations depending upon the observer's watching his own processes. Notwithstanding this skepticism, I am informed by a prominent ex-behaviorist, conclusions frequently were evolved during prolonged conversations replete with introspections but not admitted as such.

Since Lange's valuable observations on physiological phenomena associated with emotions published in 1889,[4] many investigators have added their contributions. Of particular interest have been studies of the galvanic reflex. This change in resistance to the passage of electric current attendant upon emotion has been found to depend upon the tissues within the skin; although it is not yet certain

[4] C. Lange, *Über Gemüthsbewegungen*, 1889.

whether constriction of the blood-vessels,[5] or changes in the epithelial cells,[6] or sweating[7] alone is responsible. It is agreed that the galvanic reaction depends upon the autonomic nervous system,[8] following emotion as an after effect, or at least as a late effect or manifestation. But we remain in ignorance as to what occurs in the neuromuscular system simultaneously with the emotional experience.

The aim of the present investigation is to learn something about what takes place in the nervous or neuromuscular system specifically for various forms of mental activity, and so far as possible to measure the processes in physical terms. During a period of study of relaxation covering twenty-three years, individuals who have been trained to observe their muscle sense generally have agreed in independently reporting sensations as from slight or pronounced muscular contraction marking the occurrence of mental activity. The writer has repeatedly made this observation on himself during the above-mentioned period.[9] We desire to test the possibility thus opened that the muscle sense, if carefully and critically employed, may put us on the track of observations that may be objectively verified for the whole range of our mental experience.

Accordingly in the studies here recorded, we deal with action-potentials and not with mere resistance changes, such as the galvanic reflex. These investigations, which I have been attempting to get under way since 1911, have been described elsewhere,[10] but it is believed that a résumé including certain recent results may prove of interest to students of psychology.[11]

General methods. It has been necessary to develop extremely sensitive apparatus in order to detect and measure the slight changes in potential in the nervous or muscular system during mental activi-

[5]F. Aveling and R. J. S. McDowall, Electric resistance of skin, *J. Physiol.*, 60, 1925, 316; H. B. Denscham and H. M. Wells, The mechanism by which electrical resistance of the skin is altered, *Quart. J. Exper. Physiol.*, 18, 1927, 175.

[6]C. P. Richter, A study of the electrical skin resistance and the psychogalvanic reflex in a case of unilateral sweating, *Brain*, 50, 1927, 216-235.

[7]C. W. Darrow, Sensory, secretory and electrical changes in the skin following bodily excitation, *J. Exper. Psychol.*, 10, 1927, 197; The galvanic skin reflex and finger volume changes, *Amer. J. Physiol.*, 88, 1929, 219-229.

[8]E. Schilf and A. Schuberth, Über das sogenannte psychogalvanische Reflexphänomen beim Frosch und seine Beziehung zum vegetativen Nervensysten, *Arch. f. d. ges. Physiol.*, 195, 1922, 75-95.

[9]E. Jacobson, Progressive relaxation, this JOURNAL, 36, 1925, 73-87.

[10]Jacobson, Action currents from muscular contractions during conscious processes, *Science*, 66, 1927, 403; Electrical measurements of neuromuscular states during mental activities. I, *Amer. Jour. Physiol.*, 92, 1930, 567-608; II, *ibid.*, 94, 1930, 22-34; III, *ibid.*, 95, 1930, 694-702; IV, *ibid.*, 95, 1930, 703-712; V, *ibid.*, 96, 1931, 115-121; VI, *ibid.*, 96, 1931, 122-125; VII, *ibid.*, 97, 1931, 200-209.

[11]Several suggestions regarding the writing of this résumé were kindly made by Professor Harvey Carr.

ties. Thermionic amplification has been used with the string galvanometer. In the most recent studies, measurements are possible of potential differences as small as one millionth of a volt, and such small changes are recorded with negligible lag in time on photographic film moving about 1 in. per sec. Electrodes are devised to connect the tissues with wires leading to the amplifier. Apparatus and method have been described with details that need not here be repeated.

For the most part, *S*s are employed who previously have been trained to relax.[12] This is generally necessary because restless movements occurring in untrained *S*s occasion action-potentials which obscure the readings concerning phenomena of mental activity. Relaxation, then, is requisite to furnish control tests and, in a sense, to isolate the mental activity investigated.

The *S* lies relaxed upon a couch in a darkened, quiet, partially sound-proof room. His eyes are closed. Distraction by *E* is carefully avoided. It is agreed between *E* and *S* that the clicks of a telegraph key are to be signals: the first, to engage in a particular mental activity; the second, occurring soon afterwards, to relax any muscle tensions present. By means of a signal magnet, the instant of occurrence of each click is recorded on the photograph.

Imagination of movement. To secure data for statistical consideration, one instruction is used many times with one *S* and repeated with others. *S* is to imagine bending the right arm. An imagined persistent or steady flexion of the right forearm is meant, rather than an imagined brief flexion and extension. One electrode is connected with the right biceps-brachial muscles and the other to a (partially) indifferent point—the skin or under the skin in the elbow pit.

During a period of general relaxation, when the galvanometer string with one lead attached to the right biceps is recording an approximately straight line, the signal is given for the subject to imagine what would in actual performance involve contractions in the above mentioned muscle; for instance, to imagine that he is steadily flexing the right forearm. Generally within a fraction of a second, the string ceases its steady course and engages continually or intermittently in relatively large swings which cease soon after the signal is given to relax any muscular tensions present (see Fig. 1). This test has been made upon about 20 *S*s and the results have been in agreement, with modifications later to be stated.

But under the above conditions, if it has been agreed that the first signal is to mean, "Imagine bending the left foot," the string

shows no such onset of large swings, generally continuing unaltered in its course (Fig. 2). This type of control test will be called 'Control 1.' Similarly the string continues in a steady course even after signals have been given, "Imagine bending the left arm" (Control 2), "Actually bend the left foot one inch" (Control 3), "Actually bend the left arm one inch" (Control 4), "Imagine the right arm perfectly relaxed," or, "Imagine the right arm paralyzed" (Control 5), "Imagine extending the right arm" (Control 6), and "When the signal comes, do not bother to imagine" (Control 7).

The negative results noted in the control tests rule out two possibilities: (1) that the string deflections observed during imagination are due to effects of the sound on the *S*; (2) that the act of imagination involves 'action-currents all over the body.'

The period from the beginning of the first signal to the appearance of the string vibrations (reaction-time) is often as brief as 0.2 sec. in trained *S*s, but averages 0.4 sec. in about ⅔ of all the tests on imagination of bending the right arm.

It seems unnecessary to present results in tabulated form here, since this has been done previously. That the electrical phenomena observed following the instruction to imagine bending the right arm are not artifacts due to training in relaxation has been tested with 6 *S*s not so trained. In these instances the string as a rule is not so quiet before the first and after the second signal as in trained *S*s; therefore the contrast of the vibrations occurring after the first signal with those of the foregoing and subsequent control periods is not so marked as in trained *S*s. However, after the *S* has become accustomed to conditions and has attained a fairly quiet state, tests show a definite increase in the microvoltage of vibrations with untrained as with trained individuals, excepting in instances where the instruction leads to visualization alone, as will later be discussed.

After setting the galvanometer to record higher voltages, a record is taken while the *S* actually flexes his right forearm, moving the wrist about 1 cm. The photograph quite resembles those above described for imagination of flexing the right forearm; but the microvoltage during overt movement is considerably greater than during imagination.

Imagination and recollection of various muscular acts. In a further series of investigations, the *S* is instructed as before except that he is to imagine lifting a 10-lb. weight with the right arm. Under these conditions, electrical fluctuations are found to occur in the biceps region of that arm during such imagination, but not during the foregoing and succeeding periods when the *S* is instructed to relax. Likewise they are not found to occur in the right arm when *S* imagines that he is bending his left arm, nor in the other control tests previously specified. Again it seems unnecessary to present results in statistical form, since this has previously been done.

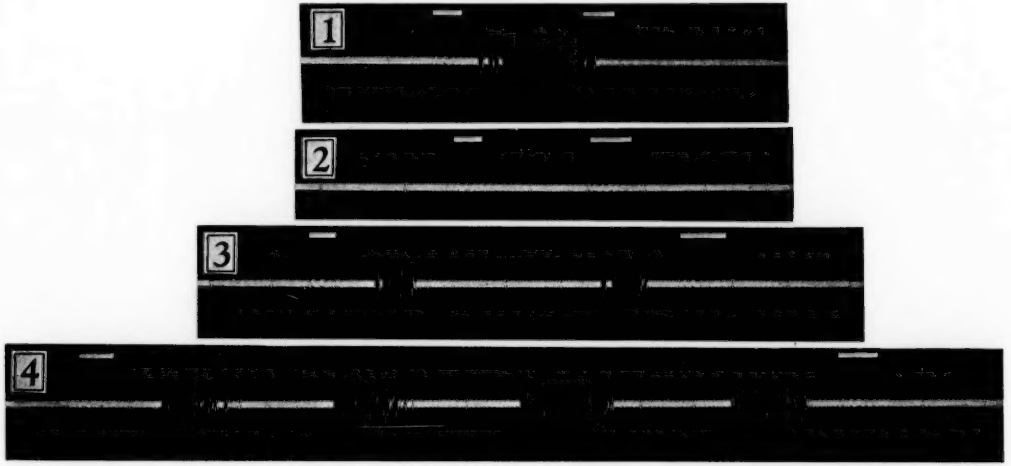
Recently this experiment has been repeated with 15 *Ss* previously trained to relax and with 12 *Ss* not trained. During imagination of lifting the weight with the right arm, action-potentials were recorded from the right biceps in 54 out of 63 tests with the trained *Ss* and in 41 out of 63 with the untrained *Ss*. During imagination of lifting the weight with the left arm, the galvanometer showed no electrical change from relaxation in the right biceps in 24 out of 24 tests with the trained *Ss* and in 20 out of 25 tests with the untrained *Ss*.

Imagination and recollection of various other acts commonly performed with the right arm have been studied with electrical apparatus. It is necessary to test for the presence of action-potentials when instructions are given which can be carried out by the *S* in terms of responses of one or another muscular group not always determinable in advance. Ideally, for these tests we should need a separate string and a set of electrodes for each important neuromuscular group, as well as a galvanometer circuit considerably freer from electromagnetic and electrostatic disturbances than was the circuit first employed; for there is always the possibility that results may be called negative through masking of very slight action-potentials by extraneous electrical disturbances.

In preliminary tests on imagination, the following instructions were selected by *E* (without consulting the *S* beforehand): "When the signal comes, imagine sweeping a room with a broom. Upon the second signal, relax any muscular tensions present." Other examples of instructions are: "Imagine writing your name;" "Imagine yourself rowing a boat;" "Imagine yourself boxing;" "Imagine scratching your chin;" "Imagine plucking a flower from a bush;" "Imagine combing your hair;" "Imagine playing the piano."

In half of these preliminary tests (14 out of 28) the string excursions were markedly increased during imagination as contrasted with relaxation; but in the other half the contrast was not marked. The *Ss* sometimes stated that following the instruction to imagine, they carried out the imagined act with some muscle-group other than the flexors of the forearm. In other instances they stated that they visualized themselves performing the act, but failed to have arm-muscle sensations. If these statements are correct, they evidently account for the absence of action-potentials from the biceps region in some of the above-described tests.

If the instructions selected by *E* fail in some instances to induce the *S* to imagine various muscular acts as carried out with his right biceps, but lead him instead to visualize or to carry out the imagination with other muscle-groups, another approach is suggested. *S* is asked to mention various acts which he would naturally perform with the right arm and would naturally imagine as so performed. In making such selection, he will be aided if he has had experience at



FIGS. 1–4. (Legends opposite)

FIG. 1. PHOTOGRAPHIC RECORD OF STRING DEFLECTIONS DURING IMAGINATION

The *S*, previously trained to relax, has been instructed, "Upon hearing the first signal, imagine lifting a 10-lb weight in the right forearm. Upon hearing the second signal, relax any muscular tensions, if present." One electrode in the biceps muscle, about 6 cm. above the other, which is inserted under the skin in the elbow-pit. Intervals between vertical time lines shown in left lower corner = 1/5 sec. Distance between horizontal lines, approximately, 1 mm. = 1.3×10^{-6} volts.

This photograph shows between signal marks (appearing at the top) groups of vertical lines, evidently due to action-potentials, which are absent in the periods of relaxation before the first signal and which subside soon after the onset of the second signal. We conclude that in the present test, muscle-fibers of the right biceps contract as the *S* imagines lifting with the right forearm.

FIG. 2. PHOTOGRAPHIC RECORD OF CONTROL TEST

Conditions the same as in Fig. 1, excepting that here the instruction has been, "Upon hearing the first signal, imagine lifting the weight with the left forearm." No evidence of action-potentials from the right biceps here appear. Results from the right biceps are negative similarly in tests where the instruction has been, "Do not bother to imagine," or "Imagine bending the right leg" (see text). The latter instruction is characteristically followed by action-potentials in the right leg.

FIG. 3. PHOTOGRAPHIC RECORD OF STRING DEFLECTIONS DURING IMAGINATION OF A REPEATED ACT

Conditions the same as for Fig. 1. The instruction has been, "Imagine hitting a nail twice with a hammer held in your right hand." Between signal marks, two series of long vertical lines, indicating action-potentials, are seen, separated by an interval of practically horizontal line, indicating relaxation.

FIG. 4. PHOTOGRAPHIC RECORD OF STRING DEFLECTIONS DURING IMAGINATION OF A RHYTHMICAL ACT

This photograph shows between signal marks four groups of vertical lines, indicating action-potentials from the flexor muscles of the right forearm, separated by approximately equal lengths of practically horizontal line, indicating relaxation.

observation of the muscle-sense. Four *S*s were used in the following tests who had such experience. In addition, all except one (*G*) had been trained to relax.

The instructions selected by them were as follows: "Imagine lifting a cigarette to your mouth;" "Imagine pulling a microscope toward yourself;" "Imagine pulling up your socks;" "Imagine grinding coffee;" "Imagine yourself chinning on a horizontal bar;" "Imagine hugging;" "Imagine shifting the gear of your automobile to first speed;" "Imagine pulling a door open;" "Imagine lifting a glass of milk to your mouth;" "Imagine climbing a rope;" "Imagine throwing a ball;" "Imagine pulling up weeds;" "Imagine pumping a bicycle tire."

Results for these 4 *S*s are positive for action-potentials from the right biceps-brachial region in 97.5% of the tests (159 out of 163). All of the control tests prove negative.

The microvoltage for such imagination, as averaged for all *S*s, is about 26. This is much less than the average value found for imagining lifting a 10-lb. weight (41 microvolts). No attempt is made to measure the microvoltages as they occur in the tissues, but only in the attached electrodes. Corrections need to be made for various errors, as previously discussed; therefore the figures presented are not final.

Beautiful records are secured when the *S* is requested to imagine some act performed rhythmically, such as "climbing a rope," "pumping a tire," or "turning an ice-cream freezer" (Figs. 3 and 4). Following the signal to imagine, the string shows a brief period of long vibrations, then an interval of rest for a fraction of a second or more, then similar periods of vibrations and rest, occurring in rhythmical succession until the signal to relax is given. On the other hand, instructions to imagine some act that involves only one relatively brief muscular action, such as "throwing a ball," typically are followed by only one brief series of long vibrations. But if the instruction is varied, "When the signal comes, imagine throwing a ball. After an interval, repeat this experience," then the record shows two periods of long vibrations, separated by an interval of rest. Corresponding results follow the instruction to imagine the act three times.

Recollection is tested like imagination. Instructions are to recall such activities as lifting a glass or a cup of tea at dinner, reading a certain paper, removing a shirt, putting on a coat or rubbers, raising the right arm, picking flowers, pulling up dandelions, pulling up an auger, playing the piano, playing the violin, boxing, punching a bag, brushing teeth, washing dishes, sweeping, running a carpet-sweeper, scrubbing, rowing, hitting a ball at tennis, fishing, landing a fish, and slinging a sledge. *S* stated before or after each test that he had previously had the experience denoted by the instruction.

If instructions are so chosen by *E* as to indicate an activity commonly performed by the right arm, we may reasonably expect that if recollection involves neuromuscular activity, some noteworthy proportion of responses will take place in the right biceps-brachial region and therefore be recorded. However, if instructions are chosen by *S* to indicate an activity which he observes that he recalls with muscle-sensations from the right arm, we should expect a higher proportion of positive results, in accordance with what was said above concerning imagination.

Following the former procedure, 7 out of 9 *S*s show action-potentials predominantly from the right biceps-brachial region; that is, in 60 out of 90 tests. The reports of the remaining 2 *S*s suggest that following the instructions they generally engaged in visual recollection alone. Perhaps this explains why action-potentials were in most tests absent for them.

In general, for any particular *S*, the microvoltages were lower and the series of action-potentials were considerably briefer during recollection than during imagination.

Evidence that specific muscles contract during imagination. The discovery that action-potentials exist in electrodes connected with specific muscles during each process of imagination or recollection leads to the question whether the fibers involved actually contract. Accordingly a lever is arranged so that under controlled conditions flexion of the right arm can be magnified about 80-fold and recorded photographically along with the electrical curve.

The results show that following the signal for *S* to imagine steadily bending his right arm or to imagine lifting a 10-lb. weight in the same manner, the lever records a flexion of the arm, generally of microscopic extent. Soon after the second signal, which signifies that *S* is to relax any muscular tensions present, the lever suddenly returns to the position it had while the arm was at rest. But in control tests, where *S* imagines bending the left arm or imagines lifting the weight with that arm, no microscopic flexion of the right arm and no action-potentials from the biceps region are recorded.

The mechanogram indicates that the maximal excursion of the arm, averaged for each *S*, varies from 0.07 mm. to 0.32 mm. In the majority of the records there is shown a practically uniform motion per sec. This ranges, in most instances, from 0.02 to 0.05 mm. per sec. Considering also other evidence in the physiological literature, we conclude that the detection of action-potentials in muscles always signifies the presence of shortening of fibers.

It is evidently desirable to include as many tests as possible bearing upon the question whether *S* can imagine flexing the right arm or lifting a weight with that arm without actually engaging in at least a microscopic flexion of the right arm. The following tests are devised so as not to depend upon the reports of *S*. (Trained *S*s all agree that in their experience it is impossible simultaneously to imagine bending the right arm and to keep it perfectly relaxed. Whether visualization can occur alone is a matter for subsequent tests.)

(*1*) *S* is instructed, upon hearing the first signal, to imagine bending the right arm or to imagine lifting a weight with that arm. Electrodes are attached to the right biceps region in the usual manner, and the lever is also attached as above described.

(*a*) In one test, the above procedure is carried out and *S* is instructed to "cease to imagine" when he hears the second signal.

(*b*) In a second test the procedure is the same, except that *S* is instructed to "relax any muscular tensions present in the right arm" when he hears the second signal.

The photographic records produced in (a) and (b) are found to be identical in type. They are entirely like those shown in Fig. 1. That is, with instructions and records as above stated, the instruction to cease to imagine is followed by the same result as the instruction to completely relax muscularly. This is evidence, secured without the intervention of the report of *S*, that in this instance ceasing to imagine is, at least in part, the same process as muscular relaxation. It supports the view that the microscopic muscular contraction is here requisite to the act of imagination.

(*2*) *S* is instructed, upon hearing the first signal, to keep the right arm completely relaxed and simultaneously to imagine bending the right arm or to imagine lifting a weight with that arm. Mechanical and electrical attachments continue as above described.

(*a*) In one test, the instruction is added that if *S* finds it impossible to imagine and to relax simultaneously as above directed, he shall at any rate engage in imagination.

(*b*) In a second test, the procedure is the same, except that here the additional instruction is that if *S* finds it impossible to imagine and to relax simultaneously as above directed, he shall at any rate keep his right arm perfectly relaxed.

The photographic records produced in (a) are entirely like those shown in Fig. 1, while those produced in (b) are entirely like those shown in Fig. 2. This indicates, without the intervention of the report of *S* but entirely in conformity with that report, that it is found impossible simultaneously to imagine bending the right arm and to keep it perfectly relaxed. Analogous results apply to imagining lifting a weight with the right arm. The photographic records indicate that *S* either imagines—whereupon action-potentials appear along with at least a microscopic flexion; or else relaxes the right arm completely—whereupon the electrical and mechanical records are negative.

The results recounted under (*1*) and (*2*) afford additional objective evidence that in the instances studied, slight muscular contraction is requisite to the process of imagination.

The galvanic reflex. Differences between the galvanic reflex (G.R.) and the action-potentials ($V°_{2m}$) here found characteristic of imagination and recollection are as follows.

(1) Voltage: G.R. has many times the voltage of $V°_{2m}$ and is commonly recorded with the D'Arsonval galvanometer, which, as commonly used, is not sensitive enough to record $V°_{2m}$.

(2) Reaction-time: an interval of 1–4 sec. or more commonly intervenes between the stimulus and the occurrence of G.R. But only a fraction of a second commonly intervenes between the signal to imagine and elongation of $V°_{2m}$. Evidence is presented that the latter phenomena are simultaneous with imagination.

(3) Frequency per second: G.R. commonly lasts considerably more than 1 sec., while $V°_{2m}$ occurs frequently per sec.

(4) Wave form: G.R. is a rise or fall of curve, while $V°_{2m}$ at usual film rates is a series of connecting almost vertical lines. Photographic records of $V°_{2m}$ during imagination resemble those taken with similar attachments when *S* bends his arm 1 cm. or more, excepting the above-mentioned difference in voltage.

(5) Direction of potential: G.R. often is chiefly unidirectional with respect to the line of zero potential. But in all instances either electrode used in recording $V°_{2m}$ is alternately positive and negative frequently per sec.

(6) Tissue of origin: G.R. arises from changes within the skin—sweat gland secretion, vascular changes, or altered permeability of cells; while $V°_{2m}$ can be recorded directly from muscular tissue, when the skin has been excluded.

(7) Stimulus: for G.R. an exciting sensory stimulus or some word is selected to arouse the emotions. The reaction is involuntary. For $V°_{2m}$, a particular instruction is given to imagine or recollect, and *S* carries it out and ceases, at will.

(8) Contraction: during imagination and recollection, there occurs a muscular contraction, practically simultaneous in duration with the action-potentials. Since the galvanic reflex is generally conceded to be due to electrical effects depending upon elements in the skin, the proof is now conclusive that the electrical manifestations ($V°_{2m}$) during imagination and recollection are not galvanic phenomena.

An objective measurement of introspection. The coincidence of the occurrence of imagination and of lengthened string vibrations is clearly brought out in certain additional tests. A signal magnet is arranged to record on the photographic record a line which can be broken when *S* presses the button of a switch with his left index finger. He is instructed to press the button during the period when imagination is taking place, releasing it at other times. After a little practice, this can be done successfully. To be sure, a perfect parallelism in time between the large linear deflections and the indicated

process of imagination is not expected. Deviations of a fraction of a second will doubtless occur, since this period of time may be required for *S* to perceive that he is imagining or has ceased to imagine. In order to gain some idea of the reaction-time of these 3 *S*s, as they lay upon the couch with eyes closed, they were requested to press the button during periods when a buzzer sounded. For all 3 the average value was 0.24 sec.

According to the signal line thus made by the *S*s in these tests, the period during which imagination takes place coincides with the period during which the string shows lengthened vibrations, except for fractions of a second at the beginning or end. As anticipated, the reaction-time of *S* in pressing the button needs to be taken into account. Fifty figures were secured for the 3 *S*s in total, representing the discrepancies in onset or conclusion between the period of lengthened vibrations and the period of imagining indicated by the *S*. The average for these figures was found to be practically identical with their average reaction-time as determined by the method described in the previous paragraph; namely, 0.23 sec. The extremely close correspondence of the latter figure to the value 0.24 previously mentioned is probably in part accidental; but making due allowance for this, the result indicates that the period of imagining coincides with the period of lengthened vibrations, subject to the slight discrepancies introduced by *S*'s reaction-time.

The procedure just described is of interest in that, viewed from a certain angle, it is an objective test of *S*'s introspective observation. So far as I know, this is the first instance on record where such objective measurement has been accomplished.

Relaxation of mental activities. Following the onset of the second signal, which signifies that *S* is to cease to imagine, there generally occurs with *S*s who have been trained to relax a distinct diminution of the large deflections within a time-interval of 0.2 to 0.5 sec. A complete disappearance of such deflections generally occurs under the same conditions within 1.5 sec. Exceptions to this generally were infrequent with the trained *S*s, but they occurred more often with 2 out of 3 of the untrained *S*s. Most notably, an untrained *S* failed to relax, as indicated by the persistence of lengthened deflections, on one occasion for 7 min. and on another occasion for more than 2 min.

The amputated limb. In the light of the foregoing observations it becomes of interest to inquire what occurs in individuals with am-

putated limbs who assert that they can do everything in imagination with the lost part that they can do with the intact part.

The left arm of a graduate student at the University of Chicago had been amputated above the elbow-joint at about the age of 8 yr. He said that he could imagine doing anything with his left hand that he could do with his right. This assertion would harmonize with our results if it meant that upon imagining acts with his left hand he merely visualizes or verbalizes the action; but not if it meant that he could imagine acts with the lost hand fully in the same physiological manner as normal *S*s.

In brief, the results with this *S* show that when he imagines bending the missing left hand, there generally occur action-potentials from the stump-biceps muscle as well as action-potentials from the muscles that flex the right hand. In other words, when this *S* engages in mental activity concerning his left hand, certain muscles contract; but these, for example in imagined flexion, instead of being merely the muscles that flex the left hand, as in intact *S*, are in the stump of the upper arm, or in the intact arm, or in both places. *S* was not informed as to the purpose or methods of the investigation. It was therefore of striking interest, when, after he had evidently engaged in subjective observation during a number of tests, he suddenly volunteered that he desired to correct his original statement that "he can imagine doing anything with his left hand that he does with his right." He now stated that when he does something with his right hand the left seems in imagination to duplicate the performance, going through the same experience. But he never has experiences of his left hand's performing any act independently of the right. He adds, "My imagination of bending the left hand is but a shadow—a duplicate of what the right hand is imagined to perform." In short, his original statement is ambiguous and he corrects it. He can imagine doing anything with his left hand that his right hand does, but only under one condition; namely, that the right hand, at the moment of the imagination, actually engages in that same act or is imagined to engage in that same act. No independent imagination, such as exists for intact *S*s, exists for this *S*'s left hand.

We find, then, that in this *S*'s mental activities involving the imagined or recalled use of the lost muscles which flex the left hand, a substitution-reaction occurs in the corresponding muscles of the right hand or in the remnant muscles of the left upper arm or in both. In normal *S*s, it was found, at least after a little practice, that mental

activities involving the muscles of the left hand are generally accompanied by no reaction in the right arm. A certain measure of independence exists for the two sides, particularly after a little practice. But this *S* apparently has lost the ability to imagine his left hand acting independently of his right hand.

Visual imagination and recollection. Visual imagination and recollection are tested with electrodes placed near the eyeballs. The circuit used yields a pattern on the photographic record distinctive for looking in each particular direction. During a period of general relaxation, when the galvanometer string is vibrating slightly and uniformly, recording a fairly horizontal band on the photographic paper, the signal is given which will incite the subject to visual imagination. Generally within a fraction of a second, the string shoots forth and back or alternately for fractions of a second or more, producing deviations from the horizontal on the photographs. These deviations cease soon after the signal is given to relax any muscular tensions present.

If the room is not too dark, *E* can generally observe that the closed eyelids of *S*s appear quiet during extreme relaxation, whereas a slight movement or convergence of the eyeballs as a rule can be detected at the moment of visual imagination. Likewise the photographic records for visual imagination show patterns resembling those above-described for eye-movements. For instance, the pattern for imagining Eiffel Tower is practically identical with the pattern of the same *S* for looking upward. Evidently in imagining the tower, the *S*'s eyeballs move upward, somewhat as they would upon actually seeing a tower. Correspondingly he reports that in imagining he looks from the base to the top of the tower.

In general, during visual imagination and recollection, we find that electrical records are secured from the ocular muscles, producing photographic patterns like those following instructions to look in one direction or another. As shown by these records, eye-movements (or convergence) characteristically occur during visual imagination and recollection.

When the trained *S* is requested to relax his eye-muscles, the curve becomes relatively horizontal and straight and, according to his report, the visual imagination or recollection disappears. Furthermore, if interrupted at any time when the curve has been relatively horizontal and straight, even if no instruction to relax has been specifi-

cally repeated, he reports that visual imagination and recollection have been absent.

Variation of specific muscles contracting during imagination. On introspective grounds, psychologists have long assumed that the same act or object can be imagined by *S* at different moments through different types of imagery. For instance, lifting a weight with the right arm might be imagined with "muscular imagery" in the right arm, or again with visual imagery. The physiologist is obliged to avoid such assumptions, but needs to develop objective tests. These are made in the present instance by attaching a set of electrodes in the ocular region of the subject, while the lever is used simultaneously to record flexion of the right forearm.

When the trained *S* is instructed "to visualize bending the right arm," voltage changes in the ocular region, as from eyeball movement, are recorded in almost all instances. But under the same conditions, action-potentials are absent from the arm-muscles in almost all instances. This indicates that the matter of bending the right arm may be imagined in at least two physiological manners: (1) in visualization—that is, with contraction occurring in eyeball muscles; and (2) in muscle sensation—that is, with contraction occurring in muscles of the right arm.

Further tests are made with electrical and mechanical recording instruments applied as above set forth, but the instruction merely is to "imagine bending the right arm," or to "imagine lifting a 10-lb. weight." While the 3 *S*s employed minutely flex the right forearm in all instances during such imagination, they visualize in some instances but do not in others, as indicated both by the objective records and by their subjective reports.[12]

In different *S*s, then, the same muscles do not always contract during the imagination or recollection of a particular act or object. But the results indicate that during imagination or recollection, muscular contraction, if absent from one region, will be found in another. It is presumed that this principle explains why the instruction to imagine using the right arm is not invariably followed by the occurrence of action-potentials in the right arm: in such instances, we assume, *S* merely visualizes the act. Further tests in this direction remain to be performed.

The speech musculature. Investigations also have been carried out on the musculature of speech during imagination, recollection and

[12] Jacobson, *Progressive Relaxation*, 1929.

abstract thinking. Since Plato called thinking "an inner speech," nothing definite has been developed on the physiology of this relationship.

Introspective accounts of Erdmann,[13] Geiger,[14] Stricker[15] and others agreed that during mental activity there commonly occur feelings like those present in the musculature during actual speech, but left in doubt whether these feelings arise from peripheral contractions. This doubt persists to date. Dodge[16] reports that after anesthetizing his tongue and lips with 20% cocaine solution, "inner speech" is not disturbed, showing its independence from peripheral elements. Mechanical methods to record movements in the speech-organs during mental activities have brought divergent results in the hands of various investigators. Positive results, at least in a considerable proportion of tests, have been reported by Curtis,[17] Courton,[18] Tomor,[19] Wyczoikowska,[20] Reed,[21] Golla,[22] and Clark,[23] but negative results by Krall,[24] and particularly by Thorson.[25] The latter points out the inadequacy of previous methods used to record movements in the speech-organs and, employing Sommer's tridimensional analyzer, obtains magnifications up to 4.5. In spite of these improvements upon previously used methods, she finds that her experiments "throw no light on the actual mechanism of internal speech," and that they show "no qualitative correspondence between overt speech and implicit speech during the verbal formulation of the same ideas." Likewise after a comprehensive review of the whole subject, Schilling finds that investigations have not yet made it possible to say conclusively as to the bodily accompaniments of inner speech.[26]

The present tests are made with electrodes in the muscles of the tongue or under-lip. Instructions are chosen to include not alone imagination and recollection, but also other types of mental activity. Briefly stated, the instructions include: to imagine counting; to imagine telling your friend the date; to recall certain poems or songs;

[13]E. Erdmann, *Psychologische Briefe*, 1851.
[14]L. Geiger, *Ursprung und Entwicklung der menschlicher Sprache*, 1868.
[15]S. Stricker, *Studien über Sprachvorstellungen*, 1880.
[16]R. Dodge, *Die motorischen Wortvorstellungen*, 1896.
[17]H. S. Curtis, Automatic movements of the larynx, this JOURNAL, 11, 1899, 237-239.
[18]H. C. Courten, Involuntary movements of the tongue, *Studies from the Yale Psychol. Lab.*, 10, 1902, 93-95.
[19]E. Tomor, Die Rolle der Muskeln beim Denken, *Arch. f. d. ges. Psychol.*, 17, 1910, 362-366.
[20]A. Wyczoikowska, Theoretical and experimental studies in the mechanism of speech, *Psychol. Rev.*, 20, 1913, 448-458.
[21]H. B. Reed, The existence and function of inner speech in thought processes, *J. Exper. Psychol.*, 1, 1916, 365-393.
[22]F. L. Golla, The objective study of neurosis, *Lancet*, 201, 1921, 116 ff.
[23]R. Clark, An experimental study of silent thinking, *Arch. de psychol.*, 48, 1922, 1-96.
[24]K. Krall, *Über unwillkürliches Flüstern*, 1925.
[25]A. M. Thorson, The relation of tongue movements to internal speech, *J. Exper. Psychol.*, 8, 1925, 1.
[26]R. Schilling, Ein Beitrag zur Funktion des Vestibularapparates, *Zsch. f. Psychol.*, 91, 1929, 204-246.

to multiply certain numbers; to think of abstract matters such as 'eternity,' 'electrical resistance,' 'Ohm's law,' the meaning of the word 'incongruous,' or 'everlasting;' and to make up your mind what you are going to do tomorrow.

When the electrodes are connected in the speech musculature of the trained *S*, the string shadow is practically quiet during relaxation. But promptly after the signal is sounded to engage in mental activity involving words or numbers, marked vibrations appear, indicating action-potentials. Soon after *S* hears the signal to relax any muscular tensions present, the vibrations cease, and the string returns to rest.

The series of vibrations occur in patterns evidently corresponding with those present during actual speech. For if the amplifier rheostat is set for a relatively low sensitivity and the instruction has been, "When the first signal comes, count aloud but as faintly as possible, 'One, two, three,' " three series of vertical lines are generally found on the photograph separated by a horizontal interval. If now the amplifier rheostat is changed to a sufficiently higher sensitivity and the instruction is this time to imagine or to recall counting but not actually to count, a quite similar photograph is secured. Likewise when verbal matters, such as a poem, are imagined or recalled, records of action-potentials are secured resembling those from actual faint speech, but of considerably less voltage.

For control tests the instructions are; (1) "When the signal comes, do not bother to imagine;" (2) "Imagine Eiffel Tower;" or (3) "Imagine lifting a 10-lb. weight with the right forearm." In the tests previously described, (1) was found to be followed by no evidence of muscular contraction, (2) by evidences of contraction in muscles that move the eyes, and (3) by contraction in muscles that flex the right forearm. But in the present tests for contraction in the muscles of speech, these three instructions afford negative results.

Following each of the objective tests here described, reports were secured in detail from 3 *S*s who had been trained to introspect. They agreed that during mental activities involving words or numbers, they feel tenseness in the tongue and lips as in saying those words or numbers, except that the feeling is slighter and more fleeting. These reports conform with our objective findings.

Conclusions

(1) When the *S*, lying relaxed with eyelids closed, engages in mental activity such as imagination or recollection, contraction

(commonly slight and fleeting) occurs in specific muscles. Evidence is thus afforded that the physiology of mental activity is not confined to closed circuits within the brain but that muscular regions participate.

(2) During visual imagination or recollection, the muscles that move the eyes contract, as if *S* were looking at the imagined object.

(3) During what psychologists term 'inner speech,' muscles in the tongue and lips contract as if to say the words in swift and abbreviated manner.

(4) During imagination or recollection of muscular acts or of matters that involve such an act on the part of the *S*, contraction occurs in some of the muscle-fibers which would engage in the actual performance of the act. Exceptions are noted when visualization occurs alone which is characteristic of some *S*s.

(5) During a particular mental activity, the muscles of a quietly lying *S*, trained to relax, remain as a rule inactive, excepting those specifically engaged as above stated.

(6) Electrical records along with subjective reports indicate that during general progressive muscular relaxation, imagery and thinking processes dwindle and disappear.

(7) Relaxation of the specific muscular contractions present during a particular mental activity brings about the disappearance of that activity. This is accomplished by trained *S*s in periods sometimes varying from about 0.2 to 1.5 sec. as measured by action-potentials.

(8) The action-potentials measured in the present investigations are proved to be very different from the galvanic reflex.

(9) A method of measuring neuromuscular processes in mental activities is presented, and the possibility of the development of a branch of study analogous with physical chemistry, and which may appropriately be called physical psychology, is herewith opened.

THE INFLUENCE OF DEGREE OF INTERPOLATED LEARNING UPON RETROACTIVE INHIBITION

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One of the functional relationships which it is important to establish in the field of retroactive inhibition is that between degree of learning and amount of inhibitory effect. It is desirable to know, not only what this relation is in cases where inhibition appears, but also whether there are degrees of learning which place the material learned beyond susceptibility to inhibition. In an experimental attack upon this problem the originally learned material constitutes one aspect with respect to which degree of learning may be varied, and the interpolated material constitutes another. The general problem, which is, experimentally, a clean cut and simple one, nevertheless raises some far reaching questions which bear upon the interpretation of results in the whole field of retroactive inhibition.

It was found in a previous study that the relative amount of retroactive inhibition varied inversely as the number of presentations given the original lists.[1] The frequency of repetition of the interpolated nonsense syllable lists was constant at 11 repetitions, while the frequencies of the original lists varied in increments of five from 6 to 26. Even with a number of presentations as high as 26, however, with the consequent overlearning, the amount of the inhibition was, in terms of recall score, 46.2%, and in terms of saving score it was 5.3%. It is evident that a high degree of overlearning does not free lists of nonsense syllables from the disintegrating effects of interpolated lists. These results at once prepare the way for a study of the effect of a variation of the degree of learning of the interpolated instead of the original lists.

The problem of the present study is to measure the influence of varying degrees of interpolated learning upon amount of retroactive inhibition, under conditions comparable to those obtaining in the previous study, when the original lists are given a constant frequency of presentation. In this experiment, as in the previous one, the methodological assumption has been made that degree of learning will increase with increasing frequency of presentation.

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[1] J. A. McGeoch, The influence of degree of learning upon retroactive inhibition, this JOURNAL, 41, 1929, 252-262.

EXPERIMENT I

Method. The same lists of 9 nonsense syllables used in the preceding experiment were employed here. They were exposed at a 2-sec. rate on an electrically driven drum and were learned by the anticipation method, the *S*s being asked to anticipate the syllables by spelling instead of by pronouncing them. Only 8 syllables were learned since, in the anticipation method, the initial syllable is a cue only and is not learned. All *S*s were given practice series on 3 days on each of which they learned a list of 0% syllables,[2] that were much more difficult than the mixed lists of the experiment proper.

The original lists were given a constant frequency of 11 repetitions, which is the same as that given the interpolated lists in the preceding experiment. The interpolated lists were given 6, 11, 16, 21, and 26 presentations, which are the numbers previously employed with the original lists. Thus, six experimental sittings were required: one rest condition and a work condition at each of the five frequencies listed.

Under the rest condition *S* was given 11 repetitions, followed by a 10-min. period of non-learning activity during which he was instructed to read silently from a book of humor, marking the three best jokes out of every ten.[3] All of the *S*s were inexperienced and would have found it difficult to maintain unaided a rest period without rehearsal, and this method was used to insure as much as possible against rehearsal. It is thought, from all the evidence available, that this method is effective. At the end of the 10-min. interval the list was relearned to 3 perfect repetitions in succession.

Under the work condition *S* was given 11 presentations, followed by a 30-sec. interval during which a second list was prepared for exposure. Immediately following this interval *S* was given either 6, 11, 16, 21, or 26 repetitions of an interpolated list. The remainder of the 10-min. period, not occupied by the interpolated presentations, was filled as under the rest condition. At the conclusion of the 10-min. interval the original list was relearned to a criterion of 3 perfect trials in succession, and the interpolated list was given one relearning presentation in order to guard against any suspicion on the part of *S* that this list was less important than the original list and hence to be studied less vigorously.

Subjects. Twelve college students, 5 men and 7 women, went through two complete cycles. That is, they went through the rest and the 5 work conditions twice, in an order which counterbalanced practice effects and positions of syllable lists. External conditions were kept at a high degree of constancy throughout.

Scoring. The data have been scored in the following ways. (1) The total number of correct anticipations has been found for both original and interpolated lists, and these numbers have been used as checks upon degree of learning. (2) Recall scores have been found in terms of the number of syllables correctly anticipated at the first relearning trial. Similar scores in terms of the second

[2] J. A. Glaze, The association value of non-sense syllables, *J. Genet. Psychol.*, 35, 1928, 255-269.

[3] It was necessary to use a 10-min. interval, instead of the 5-min. interval of the previous experiment, in order to give time for as many as 26 interpolated repetitions which, with the time for changing from one list to another, occupied about 9 min. It is unlikely that this difference should significantly affect the results.

relearning trial have also been computed, but little use will be made of them. (3) The numbers of trials required for relearning to a criterion of one perfect trial and to the more rigorous criterion of three perfect trials in succession have been found and from them saving scores have been derived. (4) Relative retroactive inhibition has been stated, for both recall and saving scores, in terms of the formula: (R−W)/R, in which R = the rest score and W = the work score.

Results. The first step necessary in the examination of the results is to inquire whether the constant frequency of 11 repetitions has been accompanied by a corresponding constancy in the number of correct anticipations. It is highly important to ascertain as accurately as possible whether the conditions have actually produced a uniform factor which may fairly be labelled constant degree of learning. The number of correct anticipations may be employed as a control measure. It is clear from Table I that there are no differences

TABLE I

MEAN NUMBERS OF CORRECT ANTICIPATIONS

(Original lists)

		Interpolated frequencies*				
	Rest	6	11	16	21	26
Cycle I	41.7	44.8	40.5	45.4	46.0	43.4
Cycle II	49.4	50.5	46.1	48.7	53.1	49.4

*The number of records included in the means is 12 for each cycle.

which are important in amount or consistent in tendency between the different means within each individual cycle. Differences of four or five correct anticipations between means for 11 trials by every one of the 12 *S*s are of such little significance that it is unnecessary to present evidence of their statistical unreliability. The means for the second cycle are uniformly higher than are those for the first, thus indicating a clear practice effect from cycle to cycle. Intra-cycle practice effects, however, have been counterbalanced. It may fairly be assumed, then, that differences between the amounts of retroactive inhibition produced under the different conditions of interpolation are not influenced to any important extent by inequalities in the learning of the original lists.

Recall scores in terms of the mean number of syllables correctly recalled (anticipated) on the first relearning trial, and the inhibition in recall, are given in Table II. The two cycles agree closely and only the relative retroactive inhibition in terms of total scores need be considered. Every interpolated frequency produces a large per-

centage of inhibition, but the 6-repetition condition yields from 19–26% less than does any of the others. The four other work conditions vary unreliably among themselves and must be considered equal.[4] The numbers of anticipations on the second relearning trial agree in tendency with the recall scores just discussed, although, as is to be expected, the amounts of inhibition are smaller at the second trial. They are, for the five degrees of interpolated learning, 31.3, 54.9, 46.4, 42.4, and 42.4.

TABLE II

RECALL SCORES AND RETROACTIVE INHIBITION
(Original lists)

		Interpolated frequencies				
	Rest	6	11	16	21	26
Cycle I	4.00	1.16	.75	.50	1.00	.66
Cycle II	5.33	2.66	.58	.91	1.00	.83
Both Cycles	4.66	1.91	.67	.71	1.00	.75
σ_M	.40	.34	.17	.17	.21	.18
% Retroaction		59.0	85.6	84.7	78.5	83.9

It may be concluded, therefore, that 6 repetitions of an interpolated list produce a degree of retroactive inhibition which, though large, is less than that yielded by numbers of repetitions from 11 to 26. Frequencies of 11, 16, 21, and 26 produce virtually equal amounts of inhibition. When interpolated learning is slight, it appears that retroaction is a function of degree of learning, but that as complete mastery is approached and passed, additional increments have no observable effect.

The results from relearning are, in implication, similar to those from recall. The data for relearning to a criterion of 3 perfect trials are the more significant (Table III). Again the two cycles are in essential agreement and there is a practice effect from cycle to cycle which is rather large in amount. Roughly half of the original trials are saved at relearning after rest, but far less than half after the work conditions. When the original lists have a frequency of 11, any frequency of interpolated learning from 6–26 has a strikingly inhibitory effect. Six repetitions produce less inhibition than do 11, 16, or 21 repetitions, which are virtually equal in effect, and than 26

[4]It can be determined by inspection that no difference between the means for these four conditions is reliable. The difference of 2.75 between the means for rest and the 6-repetition condition has a sigma of ±0.52, however. The differences between the rest condition and the other work conditions are still more reliable.

repetitions, which yield a degree of retroaction midway between that produced by 6 and by 11, 16, and 21. This decrease in retroaction from 21–26 repetitions cannot be considered significant in view of the fact that the difference between the two means concerned is 0.84 ± 2.29.[5] It must be concluded that, although amount of

TABLE III

RELEARNING SCORES AND RETROACTIVE INHIBITION
(Original lists)

		Interpolated frequencies				
	Rest	6	11	16	21	26
RL Trials, Cycle I	6.16	10.08	11.16	11.16	11.00	10.91
RL Trials, Cycle II	4.50	5.58	7.75	7.33	8.08	6.50
Both Cycles	5.33	7.83	9.46	9.25	9.54	8.70
σ_M	1.01	1.28	1.18	1.22	1.73	1.50
Saving (Both Cycles)	51.5	28.8	14.0	15.9	13.2	20.9
% Retroaction		44.1	72.8	69.1	74.3	59.4

inhibition is a function of the method of measuring retention, the influence of varying degrees of interpolated learning is in tendency the same for both recall and relearning. That is, the four higher frequencies yield nearly equal amounts of inhibition, which are less than that following 6 repetitions. The data for relearning to a criterion of only one perfect trial corroborates this conclusion.

It is necessary to inquire whether the increasing frequency of presentation of the interpolated lists has been accompanied by a corresponding increase in the number of correct anticipations. The latter may again be taken as a second measure of degree of learning and may be employed as a check upon frequency. It is evident from Table IV that the number of correct anticipations increases con-

TABLE IV

MEAN CORRECT ANTICIPATIONS
(Interpolated lists)

	Interpolated frequencies				
	6	11	16	21	26
Cycle I	15.6	42.4	75.4	114.5	144.6
Cycle II	16.4	48.0	84.6	117.8	157.7

[5]None of the differences between the means of the relearning trials under the work conditions is statistically significant; and no difference between the rest and any of the work conditions is as much as three times its sigma. In the latter case, however, it may safely be assumed that the consistency with which the means for the work conditions exceed that for the rest condition assures the validity of the tendency.

sistently and almost linearly as frequency increases from 6–26 in each cycle. One may, therefore, conclude with the more assurance regarding the influence of degree of learning, since two independent quantifications of it agree.

For comparison, the percentages of retroaction found in this and in the preceding study of degree of learning and retroaction are reproduced in Table V. It is clear from a comparison of these results that the influence of a variation in frequency is a function of the

TABLE V

COMPARISON OF THE PERCENTAGES OF RETROACTIVE INHIBITION WHEN THE FREQUENCY OF REPETITION OF ORIGINAL AND OF INTERPOLATED LISTS IS VARIED

	Frequency varied in	Number of repetitions				
		6	11	16	21	26
Recall	Original	82.2	78.4	64.8	61.5	46.2
	Interpolated	59.0	85.6	84.7	78.5	83.9
Relearning	Original	108.5	23.5	13.0	10.5	5.3
	Interpolated	44.1	72.8	69.1	74.3	59.4

particular list in the retroaction set-up to which it is applied. When a constant frequency of 11 presentations is applied to the interpolated lists and the frequency of the original lists varied as in the interpolated learning of the present experiment, retroactive inhibition decreases consistently as frequency increases. In the present experiment, however, it increases only from 6 to 11 repetitions of the interpolated material and remains virtually constant thereafter. Thus, increasing degrees of learning of the original lists free them progressively from the disintegrating effects of a constant interpolation, while an increasing degree of interpolated learning, beyond 6 repetitions, neither enhances nor diminishes the susceptibility of the original lists when learned to a constant degree. It is particularly noteworthy that the retroaction in terms of relearning remains high throughout in this case, while falling sharply in the former.

Another important factor which apparently affects amount of retroaction is the amount of practice at the experimental conditions under which, in any given case, the inhibition is being measured.[6]

[6]The appearance of a practice effect whereby inhibition decreases with repetition of the experimental conditions has been reported before. See, for example E. S. Robinson, Some factors determining the degree of retroactive inhibition, *Psychol. Monog.*, 28, 1920, (no. 128), 1-57; The 'similarity' factor in retroaction, this JOURNAL, 39, 1927, 297-312; and McGeoch, *op. cit.*

This factor may be examined in the present experiment by comparing the amount of retroactive inhibition obtained in the two cycles separately. This comparison is shown in Table VI. In terms of recall score, retroaction decreases markedly from the first to the second cycle only at the 6-repetition condition. At the other frequencies there is no marked or consistent change. In terms of relearning, however, there is a strikingly large decrease in retroaction at

TABLE VI

RETROACTIVE INHIBITION BY CYCLES

		Number of repetitions				
		6	11	16	21	26
Recall	Cycle I	71.0	81.2	87.5	75.0	83.5
	Cycle II	50.1	89.1	82.9	81.2	84.4
Relearning	Cycle I	81.1	103.1	103.1	100.0	98.6
	Cycle II	16.6	50.0	43.5	55.0	30.6

the second cycle. In percentage terms the decrease runs roughly between 50 and 68. This marked decrease in inhibition at the second cycle occurs in spite of the somewhat larger numbers of correct anticipations at this cycle, as shown in Table IV. It is worth noting that, in the second cycle, there is considerably less inhibition in relearning after an interpolation of 26 repetitions than after any of the other frequencies above 6. The difference concerned is not statistically reliable, but it suggests that, if practice were carried sufficiently far, there might be, parallel with a general decrease in susceptibility to retroactive inhibition, an inverse relation between frequency and inhibition, at least at the higher frequencies.

The influence of practice is, in this case at least, a function of the method by which retention is measured, the influence being almost absent in the case of recall and very marked in relearning. It is, also, a function of the list the frequency of which is varied. In the previous experiment, in which the frequency of the original lists was varied, a clear decrease in retroaction occurred from the first to the second cycle, when retroaction was measured in terms of recall, while almost no decrease appeared when relearning was the measure.

EXPERIMENT II

Method. In this experiment the method of complete presentation has been employed with 10 nonsense syllables constructed proportionately from the o,

6.66 and 13.33 % meaningful divisions of the Glaze list. The original lists were studied for 120 sec., while the time devoted to studying the interpolated lists was varied in steps of 30 sec. from 60 to 180 sec.

Under the rest condition the *S*s studied a list for 120 sec., were then given 45 sec. in which to write a recall, and were then allowed a period of 5 min. of no learning activity or rest. During this rest period they were asked to read silently from a humorous magazine, marking on each page the two jokes which were considered best. The *S*s considered the marking of the jokes a second part of the experiment, and their behavior while reading and marking, together with their reports at the end of the experiment, indicate that rehearsal was at a minimum. At the end of the 5 min. period a second recall was taken, for which 45 sec. were allowed.

Under the work condition the *S*s studied a list for 120 sec., were then given 45 sec. in which to write a recall, and immediately thereafter were given a second list to learn. Varying times—60, 90, 120, 150, and 180 sec.—were devoted to this interpolated learning, making 5 sub-conditions of work. At the end of the interpolated study, 45 sec. were devoted to a recall of the list studied, followed by the reading of jokes for the remainder of the 5 min. At the end of this time a second recall of the original list was taken; and then a second recall of the interpolated list in order to prevent a suspicion that it was less important than the original list and hence less to be studied.

The 45 sec. period permitted for the recalls under all conditions gave ample time for virtually all *S*s. The necessary lists, face downward and numbered, with paper for the writing of the recalls, were given to every *S* at the beginning, and the time schedule of the experiment was followed uniformly and smoothly.

Subjects. Forty-two college students, in groups of three, went through each of the six conditions—one rest and five work conditions—of the experiment in an order which counterbalanced practice effects. Every *S* had, of course, an individual copy of the lists and of the magazines used, and was seated far enough from every other *S* to prevent the giving or receiving of assistance.

Scoring. The results were scored in two ways: (1) by counting each syllable correctly recalled and in the right position as one (the *S*s were instructed to learn and recall the syllables in order); and (2) by Lyon's method which assigns partial credits in terms of both syllable and position.[7] It would be inaccurate to compute the amount of retroactive inhibition directly in terms of the numbers of syllables recalled after the interval, since this would take no account of the number originally learned. Hence the measure of retention used is the percentage which the number recalled after 5 min. is of the number recalled immediately. The percentage of retroactive inhibition is, then, the difference between the percentages of retention under rest and work divided by the percentage under rest (rest minus work over rest).

Results. The data are summarized in Table VII. Recall 1 refers to the mean numbers of syllables recalled immediately after the learning of the original lists, and Recall 2 to the mean numbers re-

[7] D. O. Lyon, A rapid and accurate method of scoring nonsense syllables, this JOURNAL, 24, 1913, 525-531.

called after the 5-min. interval. The scores in the Recall 1 column are, thus, measures of the degree of learning of the original lists under a constant learning time of 120 sec. They fluctuate somewhat, but hardly over a sufficient range to vitiate the conclusions to be drawn regarding amount of retroactive inhibition.[8]

TABLE VII

RECALL SCORES AND RETROACTIVE INHIBITION

Condition	Recall 1		Recall 2		% Recall	% Retroaction
	Av.	σ	Av.	σ		
Rest	5.404	2.25	4.238	2.32	78.42	
Work— 60 sec.	4.928	2.22	1.857	2.02	37.68	51.95
Work— 90 sec.	5.738	2.31	1.976	2.47	34.43	56.09
Work—120 sec.	4.619	2.12	1.309	1.77	28.33	63.87
Work—150 sec.	4.976	2.83	1.309	1.73	26.31	66.44
Work—180 sec.	5.238	2.27	1.738	1.80	33.18	57.68

The Recall 2 under the rest condition is clearly much greater than under any of the work conditions and this superiority is reliable statistically.[9] The work conditions do not differ by any very great amounts among themselves. The outstanding feature of the recalls under work is that the percentages of recall decrease steadily and, correspondingly, retroactive inhibition increases, from 60–150 sec. of interpolated learning. The differences involved are not reliable statistically, but the consistency of the tendency argues for the conclusion that retroaction tends to increase slowly with increase in time spent in interpolated learning from 60–150 sec. The decrease in inhibition from 150–180 sec. of interpolated learning cannot be considered established statistically. As it stands, however, it suggests the possibility that amounts of time beyond 150 sec. devoted to interpolated learning might be accompanied by decreasing amounts of retroaction. This suggestion is reënforced by the fact that, in Experiment I, there was in terms of relearning a likewise small though unreliable drop from 21 to 26 repetitions.

In order to test the possibility just mentioned, a check experiment was performed, employing the same experimental procedure and the same lists with 15 *S*s. A rest and two work conditions were used, the

[8] The results yielded by Lyon's method of scoring agree closely with those given and are not, therefore, reproduced.

[9] The difference between Recall 2 under rest and the largest corresponding score under work (at 90 sec.) is $2.262 \pm .52$. The other differences, as can be seen by inspection, are still more reliable.

latter with interpolated learning times of 150 and 240 sec. An experiment with three conditions does not exactly duplicate on a smaller scale one with six conditions, on account of the smaller opportunity for such inter-list effects as practice and negative transfer to occur. It should, however, yield tendencies which are comparable. The check experiment gives percentages of recall as follows: rest, 80.00; work, 150 sec., 28.30; and work, 240 sec., 34.61. The percentages of inhibition under the two work conditions are 64.63 and 56.74. The differences concerned are statistically unreliable, but the tendency is the same as that found in the major experiment. These results cannot be taken as final with regard to the magnitude of the decrease in retroaction as the time given to interpolated learning increases beyond 150 sec., but they are important as corroborating the fact of the decline.

It remains to inquire whether the changes in amount of inhibition have been accompanied by corresponding variations in the learning of the interpolated lists. One would expect the immediate recalls of these lists to increase as the time given to study increases. This is not the case. The means for the five study times are, in order: 4.595, 4.428, 4.571, 5.952, and 5.523. The two last, after 150 and 180 sec. of study are higher than the others, but there is a definite lack of uniformity of increase.[10] Clearly, immediate recall does not vary consistently with learning time under complete presentation, although retroaction does vary consistently from 60 to 150 sec. of interpolated learning. Evidently factors not measured by immediate recall are involved.

Problems Involved in Measuring Degree of Learning

In this paper and in the preceding one on the same general problem, degree of learning has been quantitatively controlled for experimental purposes in terms of frequency of repetition, and a second measure has been used as a check upon the methodological assumption that frequency of repetition may be used to define degree of learning. This second measure is the number of correct anticipations at each frequency. In the former experiment and in this one there has been an almost linear relation between frequency values and numbers of correct anticipations, which serves in one sense to corroborate the assumption mentioned.

[10]The slightly smaller recall after 180 sec. of interpolated learning can not certainly be looked to for an explanation of the smaller retroaction at the latter condition on account of the general irregularity of the recalls after the other study times and the corresponding regularity of the increase of retroactive inhibition notwithstanding; although the fact that the immediate recalls in the check experiment were 5.866 after 150 sec. of study and 5.333 after 240 sec. points in that direction.

When time spent in study has been used as a criterion, experimentally, of degree of learning, as in Experiment II, it has been checked by computing the number of syllables which could be recalled immediately. Interestingly enough, this by no means bears a linear relationship to time, though, even when the number recalled does not increase, degree of retroactive inhibition may. Time and number recalled are evidently different matters.

It should be pointed out that the methods used for specifying degree of learning by no means exhaust the problem; and it may also be noted that even in the apparent simplicity of the experimental procedure employed there are variables still uncontrolled, whose influence, if it exists, is entirely unknown. The latter difficulty will be discussed first and its discussion will indicate some of the other possible ways of specifying degree of learning.

(1) It must inevitably happen that, when frequency of presentation is varied, time spent in learning varies also. It is possible that a given number of correct anticipations (or any other similar criterion) might be arrived at by different *S*s after widely different numbers of trials, and hence after differing times; and it is thinkable that the variable time factor might, in itself, lead, in spite of a constant number of correct anticipations, to variable results. In these experiments, in which frequency has been varied from 6–26 repetitions and time from 60–240 sec., it may be that the results are a function of mere time spent in effort as well as of other presumably more important factors. One should measure also the influence upon retroaction of varying degrees of learning in equal amounts of time and of equal degrees of learning in varying amounts of time.[11]

(2) It is possible that, under conditions which yield imperfect learning, the percentage relation between amount learned and amount to be learned may be a factor. This is somewhat taken care of in these experiments by the fact of the linear relationship between frequency and number of correct anticipations, when the latter are plotted against the former, although its influence, if present, is unknown. In an experiment which produced roughly the same mean numbers of correct anticipations, but employed, for example, longer lists, a different result might be obtained on account of the different proportion between number of correct anticipations and total items in the list. This factor of ratio is in some sense a measure of the integration of the total list and may well be significant.

(3) Measurement, however, in terms of numbers of correct anticipations is at best rough in that it masks the distribution of the correct anticipations over the list. Thus, 40 correct anticipations of an 8-syllable list in 11 repetitions may mean 10 anticipations each of 4 syllables, 5 each of 8, or many other distributions. Quite possibly, different distributions may have different values for retention and for retroaction.

(4) It is a further possibility that the part of the learning period in which an item is first acquired (here, first correctly anticipated) may be a factor. This would be a function of the homogeneity of the list, of serial position, and of many other factors.

(5) It has already been noted that time may be a factor which operates in experiments upon degree of learning, and that the ratio between number of correct anticipations (or amount learned, in whatever terms) and the total

[11] To some extent Experiment II does the latter, since varying times spent in interpolated learning yield in some cases not very different immediate recalls.

number of items to be learned may be an important matter. Still another variable should be added, in terms of which the general problem of this paper might be attacked, viz. the criterion to which learning is carried, such as number of correct anticipations per trial, number correctly recalled immediately, a given distribution of correct anticipations over the items of the list, or number of completely perfect repetitions.

That the averaged data have produced results of a moderately high degree of uniformity is a fact—and a surprising one. It may mean that the factors mentioned are influential only in stimulating over-cautious reflections in the experimenter; or it may mean that these factors are not influential enough to dispel a uniformity which might have been much greater in their absence; or it may mean that some of them, at least, are highly correlated with frequency and hence not separable variables in this experiment. It does not mean, probably, that they can be disregarded in interpreting experimental set-ups and results.

From all of the considerations adduced, it should be apparent that the general problem of degree of learning, and the particular problem of the influence of this variable upon retroaction, have but been scratched experimentally. Within the limits of the conditions of these experiments the results are supposedly valid, but these conditions are few among many and themselves require analytic attack. Degree of learning is certainly not a unitary condition which one measures or does not measure. Rather there are many kinds of 'degree of learning.' These considerations reach beyond the present problem almost to the whole field of learning and retention. They are, moreover, of the first importance in work on retroactive inhibition, underlying as they do so much of the methodology for the study of the influence of other factors upon retroaction.[12]

Summary

When nonsense syllable lists are given a constant frequency of 11 repetitions, followed by the varying interpolations, rest, and 6, 11, 16, 21 and 26 repetitions of a second list, and then recalled and relearned after an interval of 10 min., the following results appear.

(1) The constant frequency of repetition of the original lists is accompanied by a virtually constant number of correct anticipations; and the increase in the frequency of the interpolated lists from 6 to 26 is paralleled by a linear increase in the number of correct anticipations.

(2) When retention is measured in terms of recall, six repetitions of an interpolated list produce a degree of retroactive inhibition which, though large, is less than that yielded by numbers of repetitions from 11 to 26. Frequencies of 11, 16, 21, and 26 produce virtually equal amounts of inhibition.

[12]In this paper the question whether frequency in and of itself promotes learning is left untouched. For the present purpose it matters little whether the frequencies employed or their correlated factors produce the learning.

(3) When retention is measured in terms of relearning, six repetitions of the interpolated list produce less retroaction than do 11, 16, or 21 repetitions, which are virtually equal in effect, and than 26 repetitions which yield a degree of inhibition midway between that produced by 6 and by 11, 16, and 21. On account of the unreliability of the differences, it must be concluded, however, that the four higher frequencies are nearly equal in effect.

(4) Although absolute amounts (percentages) of retroaction are higher for recall than for relearning, the percentages for both agree in tendency. That is, the four higher frequencies yield virtually equal amounts of inhibition which are less than that following 6 repetitions. Beyond a certain point which lies between 6 and 11 repetitions, additional increments have no additional inhibitory effect.

(5) A comparison of the relative amounts of retroaction obtained in this and in the former experiment indicate that retroaction is also a function of the particular list, original or interpolated, to which the variable degrees of learning are applied.

(6) The influence of practice at the experimental conditions under which retroaction is being measured is a function of the method of measuring retention and of the list the frequency of which is varied. When the frequency of the interpolated list is varied, inhibition in terms of recall shows no effect, while that in terms of relearning decreases strikingly from cycle to cycle. When the frequency of the original list is varied, recall shows a practice effect while relearning does not.

When 10-syllable lists, by the method of complete presentation, are studied for 120 sec., followed by varying interpolations, rest, and 60, 90, 120, 150, and 180 sec. of study of an interpolated list, the following results appear.

(7) Retroactive inhibition increases gradually and consistently from the 60 sec. to the 150 sec. condition and falls to 180 sec. A check experiment indicates that inhibition is less after 240 sec. of interpolated study than after 150 sec. of such study. The fact that the numbers of interpolated syllables immediately recalled are less after 180 and 240 sec. than after 150 sec. makes this decrease in inhibition hard to interpret. It can be said, however, that there is a suggestion of a decrease in retroaction beyond 150 sec. of interpolated study, but the amount of this decrease must be very small.

(8) It is significant that, throughout, retroaction is more than 50% in amount and that neither by relatively small (60 sec.) nor by relatively large (240 sec.) amounts of interpolated study can anything like complete freedom from retroactive inhibition be obtained under these experimental conditions.

The influence of degree of learning on retroactive inhibition is, thus, a function of the general method of learning, whether anticipation or complete presentation, and perhaps of other smaller differences between the two experiments reported. The experiments agree fully, however, in showing that neither very small nor very large degrees of interpolated learning, as measured, free original lists, as learned, from the disintegrating influence of interpolated acquisitions. They agree, also, in finding that a relatively low degree of interpolated learning (*e.g.* 6 repetitions and 60 and 90 sec.) produces a somewhat smaller retroaction than do relatively higher degrees of learning which are close together in the magnitude of their effects.

A discussion of the possible factors involved in the conception of 'degree of learning' indicates that it is not a unitary condition, but that there are several kinds of 'degree of learning,' all of which must be taken into account in experiments on retroaction and on retention in general.

PRESSURE ADAPTATION-TIME: A FUNCTION OF INTENSITY AND EXTENSITY

By MICHAEL J. ZIGLER, Wellesley College

The topic of cutaneous adaptation is treated in the most general way in handbooks of sense-physiology and psychology. Thunberg described Locke's classical experiment on thermal adaptation but presented no specific experimental data on adaptation to pain and pressure.[1] Titchener merely pointed out certain kinds of stimuli which may be used to demonstrate the fact of cutaneous adaptation.[2] In his survey of the experimental situation in the field of touch in 1929, Nafe treated this topic in a very general way without presenting experimental results.[3] Troland has disposed of tactual adaptation in one brief paragraph,[4] the generalizations of which are drawn from the survey of Thunberg in 1905, just cited. The fact of the matter is that extremely little experimental work has been done on cutaneous adaptation. There are a few recent studies, such as that of Straus and Uhlmann,[5] who first demonstrated experimentally the fact of algesic adaptation; that of Adrian,[6] in which he reported that the adaptation-time to painful stimulation is longer than that to non-painful stimulation; and those of Hahn,[7] which deal with the nature of thermal adaptation.

It is obvious that adaptation can be understood as the establishment of an equilibrium by the receptor, and that we have thus to

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[1]T. Thunberg, in Nagel's *Handbuch der Physiologie des Menschen*, III, 1905, 647-733.

[2]E. B. Titchener, *Experimental Psychology, Instructor's Qualitative Manual*, 1901, 81.

[3]J. P. Nafe, The sense of feeling, *Foundations of Experimental Psychology*, 1929, 392-413.

[4]L. T. Troland, *Psychophysiology*, II, 1930, 317.

[5]H. H. Straus and R. F. Uhlmann, Adaptation to superficial pain, this JOURNAL, 30, 1919, 422-424.

[6]E. D. Adrian, The impulses produced by sensory nerve endings, *J. Physiol.*, 61, 1926, 49-72.

[7]H. Hahn, Neue Anschauungen vom Temperatursinn, *Dtsch. Med. Woch.*, 53, 1927, 528-530; Über den Erregungsvorgang der Temperaturnerven, *Arch. f.d. ges. Psychol.*, 65, 1928, 41-54; Die psychophysischen Konstanten und Variablen des Temperatursinnes, *Zsch. f. Sinnesphysiol.*, 60, 1929, 198-232.

consider both adaptation during the continued stimulation and the reëstablishment of equilibrium after the stimulation ceases. These two phases of adaptation may be designated respectively, *stimulatory* and *post-stimulatory* adaptation.

In the experiments to be described here, an attempt was made to determine (1) whether the adaptation-time to continued cutaneous stimulation varies with the intensity and extensity of stimulation, and (2) whether the adaptation-times for continued stimulation and for the reëstablishment of normal equilibrium afterward are essentially of the same magnitude or different. Experiment I deals with the first problem, and Experiment II has to do with the second.

Experiment I

After preliminary trials with cork stimuli, we constructed an intensity-set of five stimuli, and an extensity-set of the same number of stimuli. All were made of composition metal. All members of the intensity-set had the same diameter, 1 cm., and weighed respectively 50, 100, 500, 1000 and 2000 mgrm. All members of the extensity set weighed the same, 500 mgrm., and measured respectively 5, 10, 15, 20 and 25 mm. in diam. These cylindrical weights were applied to the skin manually by short threads which were fastened at the edges of the weight and drawn together above so as to insure uniform manual application. In order to eliminate thermal stimulation, in the case of the intensity-set, a small disk of paper was placed on the skin long enough for complete adaptation to take place before the weight was placed upon it. In the work on extent, the *E* held the set of weights on one hand so as to keep them warm. A haphazard order of presentation was followed in both cases.

The trials with the intensity-stimuli were made in four regions: (1) the back of the hand between the second and third metacarpals, (2) about 3 in. above the carpal folds, on the volar surface of the forearm, (3) the middle of the forehead, and (4) the cheek. The trials with the extensity-set of stimuli were presented in two regions: (1) the center of the forehead and (2) the cheek. For the trials on the hand and forearm, *O* sat at a low table, but for the work on the forehead and cheek he took a supine position. At no time was *O* permitted to see the weights.

At the moment when a stimulus was applied to the skin, a stop-watch was started so as to measure the interval of time between the application and the time when *O* indicated verbally that the stimulus was no longer felt.

The *O*s analyzed the period of adaptation into three phases. First, there is the brief period of application, which consists of light impact over a field of pressure of quite clearly defined outline. This is followed by a period in which the outline grows vague and dim and the field of pressure undergoes diminution in size until only a small point of pressure remains. During this second phase *O* reports regularly that it appears as if the stimulus were gradually being lifted from the skin. The third phase begins when *O* experiences uncertainty as to whether stimulation is still perceptible. The *O*s generally indicated that adapta-

tion does not terminate suddenly, but gradually within an interval of time in which uncertainty regarding continuation of stimulation prevails. With training *O* establishes a subjective standard by which he is willing to report when stimulation ceases.

Table I contains the average adaptation-times (A.T.) in seconds with mean variations (M.V.) for eight *O*s in the four regions of stimulation by the intensity-series of stimuli. Table II includes the same data for stimulations in two regions with the extensity series of stimuli. The averages in the table for intensity are based on 24 trials in the case of all *O*s, except Z, who furnished 30. Those in the other table are in every case computed from 12 sets of trials.

Inspection of Table I reveals the fact that adaptation-times to stimulation by heavier weights tend to be longer than those to stimulation by lighter weights. It may also be observed that the duration varies with the relative intensity of stimulation. Although inversions appear in certain series of averages, they tend to be insignificant. The tendency for adaptation-time to vary with intensity of stimulation is manifested more or less clearly in the averages of every *O*. The inversions which appear in the series of certain *O*s are leveled out completely when the averages of all *O*s are combined.

Table II indicates a tendency in the opposite direction, namely, for adaptation-time to vary inversely with the area of stimulation. There are several inversions here again, but the tendency may nevertheless be seen, and in the combined averages it is clearly evident. The averages in this table do not show quite the width of distribution generally manifested in Table I. It may be that our steps were too small, for the *O*s reported that differences in size are less clearly observable than those of intensity. This probably also explains the fact that relatively a larger number of inversions appear in Table II than in Table I.

It may be pointed out that such variation in extensity can be resolved in reality to a special case of variation in intensity. With the weight of largest diameter, for example, 500 mgrm. are distributed over relatively a larger field than with any of the other weights, where the same amount of weight is supported by a smaller portion of the skin.

There is little justification for asserting an exact functional relation between adaptation-time and intensity or extent. There is a possibility that a linear relation is masked by a large variability. This variability probably signifies that the judgments were subject to a number of such variable errors, as temporary bodily condition,

TABLE I

ADAPTATION-TIMES (SEC.) FOR PRESSURE WHEN THE INTENSITY OF THE STIMULUS IS VARIED AND THE EXTENSITY IS CONSTANT

	Stimulus	Observers																		Combined Av.
		B		*D*		*E*		*G*		*M*		*P*		*W*		*Z*				
	(mgrm.)	A.T.	M.V.	A.T.	M.V.	A.T.	M.V.	A.T.	M.V.	A.T.	M.V.	A.T.	M.V.	A.T.	M.V.	A.T.	M.V.		
Back of Hand	50	1.5	1.0	.8	.4	2.0	1.1	2.2	.2	4.2	1.6	1.0	.4	.7	.4	7.0	2.5			2.42
	100	2.2	.8	1.0	.6	2.8	1.6	3.0	.5	6.3	1.8	2.5	1.2	1.6	.8	10.7	4.1			3.82
	500	4.3	1.5	1.2	.8	4.3	1.7	6.1	1.2	9.3	1.8	5.9	2.5	4.6	1.2	13.0	5.3			6.01
	1000	4.6	1.8	1.2	.8	5.2	1.8	7.0	1.2	9.8	2.4	6.8	2.6	5.5	2.0	13.6	5.2			6.71
	2000	6.5	2.0	1.4	.6	10.2	6.3	11.0	1.5	14.1	2.6	9.9	3.4	5.8	1.6	17.3	5.4			9.52
Forearm	50	2.3	1.4	.9	.3	1.4	.6	.7	.4	4.0	.5	3.1	1.4	.6	.2	5.5	1.3			2.31
	100	3.0	1.5	1.0	.5	2.2	1.1	2.1	1.2	5.8	1.6	4.6	1.3	1.0	.5	6.6	2.4			3.28
	500	4.1	1.8	1.1	.7	4.0	2.1	4.7	1.6	6.7	1.4	7.5	2.3	2.6	1.2	8.2	3.0			4.86
	1000	4.3	1.6	1.4	1.2	4.4	2.2	5.8	2.2	8.8	2.4	8.5	3.0	3.1	1.4	8.5	3.2			5.60
	2000	7.3	2.2	1.6	1.0	6.8	3.3	9.2	2.4	10.8	3.2	11.5	5.4	3.5	1.6	11.4	4.5			7.76
Forehead	50	4.2	2.6	.8	.4	2.2	1.3	10.6	2.4	5.6	3.0	8.9	3.5	1.6	1.2	6.7	1.0			5.07
	100	4.3	2.4	1.1	.6	2.9	1.6	11.1	2.2	7.4	2.2	12.2	5.6	2.5	1.5	8.3	3.4			6.22
	500	8.3	2.7	1.5	1.0	5.7	2.2	20.9	3.4	10.4	3.2	15.9	6.1	4.5	2.5	12.5	4.2			9.96
	1000	9.8	3.6	1.6	1.2	6.7	2.9	24.0	4.5	12.5	3.5	19.1	7.5	6.2	3.4	13.6	5.4			10.43
	2000	13.1	3.5	1.8	1.2	8.9	3.6	35.0	6.0	19.1	2.4	21.8	8.2	8.9	4.5	19.7	6.7			16.03
Cheek	50	4.6	2.3	.8	.4	2.0	1.0	7.5	1.0	5.1	1.7	18.8	3.3	1.3	.7	5.6	2.1			5.71
	100	5.5	3.0	1.1	.8	2.3	1.2	13.1	2.2	7.6	3.2	12.9	3.6	1.5	1.0	7.0	3.2			6.37
	500	11.6	3.4	1.5	.5	5.2	3.0	30.7	4.4	12.7	3.8	17.3	5.4	2.6	1.3	11.5	4.7			11.63
	1000	13.4	3.8	1.6	1.2	5.3	2.9	33.0	6.0	15.8	3.6	24.5	7.0	4.0	2.0	10.5	3.2			13.51
	2000	18.5	4.0	1.8	.8	8.9	5.1	53.2	5.4	23.3	3.7	30.3	10.3	4.7	2.7	14.2	4.5			19.36

TABLE II

ADAPTATION-TIMES (SEC.) FOR PRESSURE WHEN THE EXTENSITY OF THE STIMULUS IS VARIED AND THE INTENSITY IS CONSTANT

	Stimulus (mm. diam.)	Observers																		Combined Av.
		B		*E*		*H*		*M*		*N*		*R*		*Y*		*Z*				
		A.T.	M.V.	A.T.	M.V.	A.T.	M.V.	A.T.	M.V.	A.T.	M.V.	A.T.	M.V.	A.T.	M.V.	A.T.	M.V.			
Forehead	5	15.5	3.7	13.8	2.5	22.5	5.2	16.6	6.8	11.7	1.4	33.0	7.6	13.2	4.2	24.6	5.4	18.86		
	10	12.0	4.0	12.2	2.4	17.2	6.8	10.2	3.8	10.0	1.5	32.8	7.3	14.2	3.7	20.5	5.0	16.13		
	15	11.5	3.0	7.4	.8	13.5	3.9	15.6	3.6	9.0	2.7	24.2	9.6	16.2	4.5	19.0	4.3	14.55		
	20	10.0	3.1	5.0	.6	9.5	4.6	17.2	7.0	7.2	.6	19.8	8.9	14.2	3.3	15.7	5.5	12.32		
	25	8.0	2.5	4.0	1.2	7.2	2.0	10.2	5.8	5.5	.7	17.6	6.4	12.7	2.8	13.8	3.7	9.87		
Cheek	5	32.3	9.3	18.2	3.1	17.4	2.4	24.8	9.7	17.7	3.7	24.2	5.0	13.4	2.9	22.0	7.3	21.25		
	10	20.0	6.5	12.0	3.5	13.1	2.4	16.8	9.6	15.2	2.2	19.0	7.6	15.0	4.7	20.5	6.4	16.45		
	15	16.2	3.9	8.4	1.6	11.6	2.6	15.8	9.3	13.5	2.6	18.8	6.2	16.0	4.7	15.0	4.5	14.41		
	20	12.8	4.4	5.8	.6	8.0	3.0	9.0	5.6	11.8	2.4	22.4	7.0	16.0	3.0	12.0	2.0	12.22		
	25	7.6	2.4	3.8	.9	5.6	.8	7.6	2.4	8.7	2.2	21.4	3.9	15.5	4.5	12.5	3.0	10.33		

fatigue, shift in attitude, spontaneous intrusion of other cutaneous experiences, alteration of the subjective standard as to when adaptation is complete.

With regard to the comparative adaptation-times in the different regions of the skin, the results are not conclusive. Four *O*s, *D*, *E*, *W* and *Z* gave adaptation-times of approximately the same magnitude in all regions of stimulation, but the other *O*s reported definitely longer adaptation-times in the face regions. If all cutaneous receptors have about the same duration of adaptation, and certain *S*s report differences in adaptation times, this may mean that in some *O*s the receptors are overlaid by thicker and tougher integument in certain parts; but if it is eventually to be discovered that the receptors in certain regions have longer adaptation-times, we are then merely in position to conclude that the difference is considerable in some cases, and that in other cases the difference is so small as to be obscured by the variable errors which play a rôle in the experimental situation.

There are marked differences in the adaptation-times of different *O*s. In the same region of the skin, one *O* requires from 8 to 15 times longer for complete adaptation than another. Some *O*s furnish short adaptation-times in all regions, others comparatively long in all regions, and still others comparatively short on the limb and comparatively long on the face.

Upon learning that both of us had started experiments in cutaneous adaptation, Professor John P. Nafe of Clark University and the writer arranged to meet several times to observe in a preliminary way for one another. Although his work had to do with partial adaptation, while ours was concerned with complete adaptation, he found it convenient to set two of his undergraduate students, Thomas Carleton and Francis Singleton, upon the problem of the relationship of intensity and complete adaptation-time. Professor Nafe has kindly turned over their results to me.

Five weights were prepared, consisting of regulation shotgun shells loaded with small lead. They weighed respectively 8, 28, 48, 68 and 88 grm. Carleton and Singleton observed for one another, presenting the weights in haphazard order, and measured the periods of complete adaptation with a stopwatch. Over *O*'s arm, which was bared and placed with volar side up in a comfortable position on a table, projected the arm of an upright stand. In the end of this arm was a circular opening of a diameter slightly larger than that of one of the shells. The stimulus was dropped gently through this opening upon a small cardboard disk, which had been placed upon the skin previous to the trial in order to rule out thermal stimulation and to insure the more uniform distribution of stimulation. Care was taken to make certain that adaptation to the cardboard disk had been complete before a weight was applied.

After preliminary trials, each of the 5 weights was presented 25 times (1) on the volar surface of the arm and (2) on the back of the hand. Only 5 applications of each stimulus were made at each sitting. The position of application was shifted slightly to avoid local fatigue.

The results are presented in Table III. They show in a convincing manner that adaptation-time varies directly with intensity of stimulation. One *O* consistently had shorter adaptation-times than the other. The steps in adaptation-

TABLE III

ADAPTATION-TIME (SECS.) FOR PRESSURE WHEN THE INTENSITY OF THE STIMULUS IS VARIED

Carleton and Singleton's results

			STIMULUS (grm.)				
		O	8	28	48	68	88
Arm		*C*	3.22	6.97	10.25	12.03	18.86
		S	12.14	13.63	18.62	25.00	26.22
		Av.	7.68	10.30	14.43	18.51	22.54
Hand		*C*	4.26	9.24	16.46	22.21	25.87
		S	5.94	11.26	16.84	28.50	30.71
		Av.	5.10	10.25	16.65	25.35	28.29
	Combined Average		6.39	10.27	15.54	21.98	25.41

times for each *O*, corresponding to the step increments in intensity, are not quite uniform; nevertheless, there is a rough approximation to equal steps of increase. The combined average of the two regions shows a good approximation to equal steps. This result would seem to suggest the possibility that the same increment of intensity (perhaps at any point on the intensity continuum, other things being kept the same), will require the same amount of additional time for complete adaptation, for there is apparently a linear relationship between intensity and adaptation-time.

EXPERIMENT II

In this experiment we proposed to compare the adaptation-time to continued stimulation with the adaptation-time which follows the removal of a stimulus. If adaptation consists in the establishment of a condition of balance or equilibrium by the receptor, it may be that the phase of adaptation which takes place during the continued application of a stimulus is the complement of that which follows the removal of that stimulus.

To test out this hypothesis, we performed experiments in four regions of the skin, using our 5 intensity-weights (*vide supra*) as stimuli. Under the same experimental conditions which prevailed in the earlier experiments, we required our *O*s now to report (1) the moment, following the application of a weight, when

stimulation first ceased to be perceptibly effective (whereupon the weight was promptly removed by *E*), and (2) the moment at which post-stimulatory adaptation was complete, that is to say, when the after-effects attendant upon the removal of the stimulus had disappeared. Time permitted us to present each of the 5 stimuli only 8 times in the case of two *O*s, *E* and *Z*, only 4 times in the case of *W*, and only twice in the case of *G*. For the last named *O* we did not calculate the mean variations.

The averages for both types of adaptation-time, (S. A.) stimulatory and (P.A.) post-stimulatory, are presented in Table IV. The measurements are in seconds.

The averages of 3 of the 4 *O*s indicate that the adaptation-time following the removal of a stimulus from the skin is roughly of the same magnitude as that for adaptation to the continued application of that stimulus. This probably means that stimulatory and post-stimulatory adaptation represent essentially reciprocal changes in the receptor.

G reported much longer post-stimulatory adaptation-times. She may represent a special case in which the two kinds of adaptation require different amounts of time. However, it is possible that her results might have been more like those of the other *O*s, if time had permitted more trials to be made. In fact, there is evidence to support the inference that this *O*, to a degree less manifest in the case of the other *O*s, failed to maintain in this experiment the same attitude that she had in the previous one. At least, if the results for stimulatory adaptation-time which are given in Tables I and IV be compared *O* by *O*, a greater lack of agreement appears in the results of *G* than in those of any other *O*. In view of the fact that *G* is an assistant in the laboratory, the possibility exists that her knowledge that after-sensations follow the removal of a cutaneous stimulus may have predisposed her to report more delayed adaptation-times in this case. It may be noted, furthermore, that in her case the adaptation-times for the different intensities are less scattered, that is to say, they fail to show as definite variation with intensity as do the results of the other *O*s.

The duration of after-sensations (or after-images) of touch have been measured by Spindler,[8] Hayes,[9] and Dimmick,[10] and each of these investigators presents results which exceed considerably our figures for post-stimulatory adaptation-time. Yet, it is clear that the experiences of pressure during post-stimulatory adaptation are genuine after-sensations. But in dealing with the same sensory datum, on the one hand in experiments on after-sensations, and on the other in experiments on adaptation, the situation of the observer

[8]F. N. Spindler, After-sensations of touch, *Psychol. Rev.*, 4, 1897, 631-640.
[9]M. H. S. Hayes, A study of cutaneous after-sensations, *Psychol. Monog.*, 14, 1912, (no. 60), 1-89.
[10]F. L. Dimmick, On cutaneous after-images, this JOURNAL, 27, 1916, 566-569.

TABLE IV

ADAPTATION-TIMES (SEC.) FOR PRESSURE WHEN INTENSITY OF STIMULUS IS VARIED

S.A. = stimulatory adaptation-time or the time for complete adaptation while the stimulus is continued. P.A. = post-stimulatory adaptation-time or the time for the recovery of the original equilibrium after the removal of the stimulus and thus of the fading out of the after-sensation

O	Stim. (mgrm.)	Back of Hand				Forearm				Forehead				Cheek			
		S.A.	M.V.	P.A.	M.V.	S.A.	M.V.	P.A.	M.V.	S.A.	M.V.	P.A.	M.V.	S.A.	M.V.	P.A.	M.V.
	50	2.0	.75	3.0	1.3	1.2	.42	1.8	.95	2.5	.75	3.6	2.1	2.6	1.3	3.8	1.2
	100	3.1	1.2	5.0	2.2	3.0	1.5	4.7	2.3	4.1	2.1	9.0	2.2	3.1	1.8	3.7	1.4
E	500	5.2	1.6	6.6	3.6	5.1	2.9	6.5	3.8	7.5	3.0	7.0	1.5	6.8	2.8	7.0	3.1
	1000	7.3	2.3	7.6	3.1	7.1	3.6	8.1	2.4	7.5	2.0	6.9	1.9	9.5	5.0	8.1	4.6
	2000	9.5	4.5	11.5	6.0	10.3	3.8	9.3	3.8	9.2	4.0	9.0	3.0	15.0	7.0	10.8	2.7
	50	2.5		40		4		30		5		27		6		24.5	
	100	11		45.5		11		17.5		5.5		45		10.5		28	
G	500	20.5		46		19.5		27.5		17		36		18		31.5	
	1000	21		46.5		11		28.5		30		38		27.5		39.5	
	2000	40.5		38		62		36		45		51		29		41	
	50	1.2	.35	2.5	1.5	1.0	.25	2.5	1.0	2.5	1.0	4.2	1.7	.75	.15	1.2	.42
	100	1.7	.6	2.7	1.1	1.0	.25	1.7	.60	1.2	.35	2.5	1.0	1.5	1.0	3.2	1.2
W	500	2.0	1.0	2.7	.85	4.0	2.5	4.0	1.5	4.5	1.5	5.7	1.3	2.5	1.0	3.7	1.1
	1000	4.2	2.1	7.0	2.2	4.7	1.9	7.7	1.1	7.0	3.0	7.2	2.6	8.5	3.0	9.0	2.5
	2000	8.7	2.1	10.0	3.0	7.2	3.3	7.5	2.0	8.0	3.2	8.7	4.1	10.5	3.5	11.0	2.5
	50	6.3	1.3	7.5	2.0	5.6	.97	6.0	.5	7.8	1.6	8.2	2.5	6.8	2.1	6.8	2.3
	100	10.2	2.1	9.7	2.0	6.0	1.0	5.7	1.5	8.8	2.1	9.6	2.1	8.8	4.3	9.6	3.5
Z	500	11.0	3.0	10.8	2.8	8.0	1.6	8.0	2.5	16.5	2.5	18.0	6.5	14.2	4.2	13.2	4.5
	1000	12.3	4.4	13.3	5.1	8.6	2.7	10.0	1.7	20.0	6.7	18.1	5.9	14.1	3.1	12.8	4.6
	2000	15.6	2.6	13.8	2.0	10.1	2.6	11.1	3.9	24.7	7.2	22.3	8.3	16.7	3.3	13.8	2.8

may be different. In the case of adaptation experiments, there is probably less likelihood that subjective touches will dove-tail into the waning sensory experience, and the suggestion that the after-sensation should continue for some time may be stronger than that adaptation should consume considerable time.

A closer inspection of Table IV reveals the fact that the average post-stimulatory adaptation-times of *E* and *W* are in most cases slightly greater than their average times for stimulatory adaptation. In the case of *Z*, 9 of the 20 post-stimulatory averages are slightly shorter, and in 2 other cases the measurements are just the same. In view of the fact that, in general, these differences are comparatively insignificant, it seems justifiable to infer that whatever discrepancies the table reveals in this respect are to be attributed to the accidental influences which play a rôle in such an experimental situation.

Theoretical Considerations

The implications of these experimental results should be adapted to the prevalent views in regard to cutaneous theory. Since the *all-or-none* law has gained widespread acceptance, the qualitative type of theory, which implies the hypothesis of the *specific energy of nerves* has been largely superseded by quantitative theories. Nafe has recently advanced such a theory of "feeling."[11] In demonstrating that adaptation-time is directly proportional to the intensity or amount of stimulation, and in indicating that in all probability a linear relationship prevails between these two functions, we have shown that adaptation represents a quantitative continuum, and thus have contributed to the support of that theory.[12]

As to what happens in the cutaneous receptor when adaptation to pressure is in progress, we are probably not yet in possession of all of the facts necessary to arrive at a positive conclusion; however, certain available facts indicate the general nature of the function of adaptation.

It has been suggested that adaptation represents the establishment of a state of equilibrium by the receptor. Stimulation is said to disturb the equilibrium of the receptor, and, if the same amount of

[11]J. P. Nafe, A quantitative theory of feeling, *J. Gen. Psychol.*, 2, 1929, 199-211; The sense of feeling, *Foundations of Experimental Psychology*, 1929, 392-413.

[12]Straus and Uhlman, *op. cit.*, reported that adaptation-time of the receptors of the skin varies with intensity in the stimulation of pain, a result that resembles the present finding for pressure.

stimulation be continued long enough, equilibrium is reëstablished by the receptor so that the stimulation becomes ineffective. What, most probably, is the nature of these changes in the equilibrium of the receptor during the time that stimulation and adaptation are in progress?

Owing to the fact that adaptation-time to more intense stimulation is longer than that to less intense stimulation, adaptation obviously cannot consist in a fatigue-producing process in the receptor.

The writer believes that the essential nature of adaptation and the related functions of the receptor may be explained by the bipolar theory recently elaborated by Crile.[13] According to this view, the receptor represents a bipolar electro-chemical unit. In the resting condition, it maintains a state of normal electrical potential. Under stimulation this normal condition is broken down, the degree depending upon the intensity of stimulation. The bipolar unit (or receptor) acts in essential respects like a self-charging condenser. It transforms energy and discharges it in impulses over the nerve. These impulses vary in frequency, depending upon the intensity of stimulation. But if the same stimulus continues to be applied, a recharging process is initiated by the receptor so that in time the normal electrical potential (or equilibrium) is reëstablished. The length of the recharging period is also a function of the intensity of stimulation. A greater amount of stimulation breaks down the electrical charge to a greater degree than one of less intensity, accordingly, a longer period of time is necessary for the normal electrical potential to be reëstablished in the electro-chemical unit.

The careful removal of a stimulus, to the continued application of which the receptor has already established complete adaptation, sets up stimulating conditions of essentially the same degree as the stimulation which was occasioned by that stimulus when it was first applied to the skin. The adaptation-time is, accordingly, approximately the same in both cases.

In the stimulation of an aggregation of receptors, as in our experiments, the same principle of operation probably holds as that which operates when an individual receptor is activated. Although the central nervous system coöperates with the activated receptors in the transaction of these functions, we are not yet in possession of a body of facts sufficient to enable us to infer how and to what degree it plays a rôle in adaptation.

[13] G. W. Crile, *A Bipolar Theory of Living Processes*, 1926, 405.

Summary

Experimental results are presented, in the first part of this paper, which show that adaptation-time to cutaneous stimulation is directly proportional to the intensity of stimulation, and inversely proportional to the extent of the field of stimulation. It is indicated that the relationship existing here is probably linear.

In the second part of the paper, experimental results are produced, which indicate that the adaptation-time to the continued application of a stimulus to the skin is approximately of the same magnitude as that which follows the removal of that stimulus, after complete stimulatory adaptation had been established.

These results are interpreted as signifying that the cutaneous receptor for pressure may act in its essential operations like a bipolar electro-chemical unit. It maintains, in the rested condition, a certain electrical potential, which is broken down under conditions of stimulation, the degree varying directly with the intensity of stimulation. As long as the electrical potential of the receptor remains below normal level under stimulation, the receptor discharges impulses over the nerve. Nevertheless, the bipolar unit (or receptor) is capable of reacting in such a way, even while uniform conditions of stimulation are sustained, that its normal electrical charge is restored. This function corresponds to adaptation, for when there is complete restoration of the electrical potential in the unit (or receptor), it ceases to discharge impulses; in other words, the continued stimulation becomes ineffective.

EFFECT OF COMPLEXITY OF MATERIAL UPON THE FORM OF LEARNING CURVES

By J. B. Stroud, Kansas State Teachers College of Emporia

In a recent study the writer obtained s-shaped learning curves for poetry.[1] In seeking an explanation of this phenomenon two conditions incident to memorizing poetry seemed sufficiently suggestive to merit further study. These are 'serial order' and 'size of unit of response.' Practically all the *S*s tended to learn the lines in serial order. Inasmuch as they read the entire selection aloud each trial they were naturally required to give some attention to the whole poem, with each reading, while probably making a special effort to remember certain lines. Thus it seemed logical to assume that, while *S* was fixating the initial lines, he would acquire increasing familiarity with the subsequent lines, and would, therefore, be able to learn them with increasing speed, within limits, when he came to make a more direct effort to learn them. Such factors would tend to account for the initial positive acceleration of s curves.

In the study reported above, the unit of response was a line of poetry. If, for example, on a given trial *S* repeated 5 lines correctly he would be credited with 5 correct responses. It seemed that under this plan the *S* might be able to reproduce relatively few whole lines during the initial trials, although he might be able to recall several part lines without credit. This factor would also help to account for initial positive acceleration. The results of the present study indicate that this formulation was too restricted. The experiments reported here were undertaken in order to determine whether the two factors named above are responsible for the s-shape of the curves.[2]

Experiment I

Apparatus and procedure. The learning material used in this experiment was monosyllabic nouns. The more obvious meaningful combinations were avoided. Two lists of such words were prepared. Both lists were learned by the method of

*Accepted for publication March 23, 1931.

[1] J. B. Stroud, Learning curves for poetry, this Journal, 43, 1931, 684-686.

[2] The writer wishes to thank Professor H. A. Carr of the University of Chicago for his helpful criticisms.

direct recall, but under two different conditions. Under Condition A the *Ss* were instructed to learn the words in any order they wished, and under Condition B they were instructed to make an effort to learn the words in serial order. This variation was designed to test the influence of serial order upon the form of the learning curve.

The size of the unit of response was also varied by using 4 different sets of words of varying degrees of complexity. Set *a* consisted of 12 words taken one at a time; Set *b*, 24 words taken two at a time; Set *c*, 36 words taken three at a time; and Set *d*, 48 words taken four at a time. Thus the number of units was constant in every set but the size of the units varied.

Four groups of *Ss* were used. Group I learned a list of words of Set *a* according to Condition A and another list according to Condition B. Group II learned one list of words of Set *b* according to Condition A and another list according to Condition B. Similar procedures were followed in the case of Groups III and IV with Sets *c* and *d*. A counterbalanced order was used both as to lists and conditions. Since a different group of *Ss* was used for every one of the four sets of words, practice did not affect the results.

Readings and recalls were alternated. One reading and one recall constituted a trial. In both learning conditions (A or B), the *Ss* were credited with a correct response for each unit correctly recalled. A unit was considered correct when all the words in it, in case there was more than one word, were recited in the order in which they occurred. A list was considered learned, under Condition A, when all the units were recalled, regardless of the order of the units, and under Condition B when all the units were recalled in serial order.[3]

The units recalled were plotted against trials, in order to obtain the learning curves. The responses were recorded in such a way that it was possible also to plot curves in terms of words against trials as well as units of words against trials.

Where several records are averaged so as to make a composite learning curve, it is imperative when the shape of the curve is under consideration, that all the individual curves be reduced to a common length before averaging them. All the individual curves obtained in these experiments were reduced to 10 equal parts. The actual number of trials varied from 4-25. This particular division seemed sufficient to preserve most of the individual characteristics of the curves and at the same time afforded a convenient working basis. The curves were equated graphically by the method illustrated in Fig. 1.[4]

As illustrated in Fig. 1, a curve was plotted in the usual way for each individual record, the points being connected by straight lines. Each baseline was divided into 10 equal parts comprising what may be termed an abstract baseline as 'a,' Fig. 1. Ordinates were erected from each division point on the new baseline to the intersection with the curve. It was possible to determine the height of the curve at each intersection point by reading the corresponding values on the ordinate as illustrated by x and y. The height of the curve at the intersection of x and

[3]The words in the respective units had, of course, to be in order. In practically all cases, however, the *Ss* learned the units in serial order. After the first few readings most of them adopted the condition of serial order, as if to take advantage of place associations.

[4]C. L. Kjerstad, The form of the learning curves for memory, *Psychol. Monog.*, 26, 1919, (no. 116), 25-27, appears to have used a somewhat similar method.

y is 2.6. When the curves are plotted on large surfaces they may be equated by this method accurately and conveniently. This procedure usually has the effect of smoothing the curves slightly, but this does not appear to be a limitation when the form of the curve is being studied.

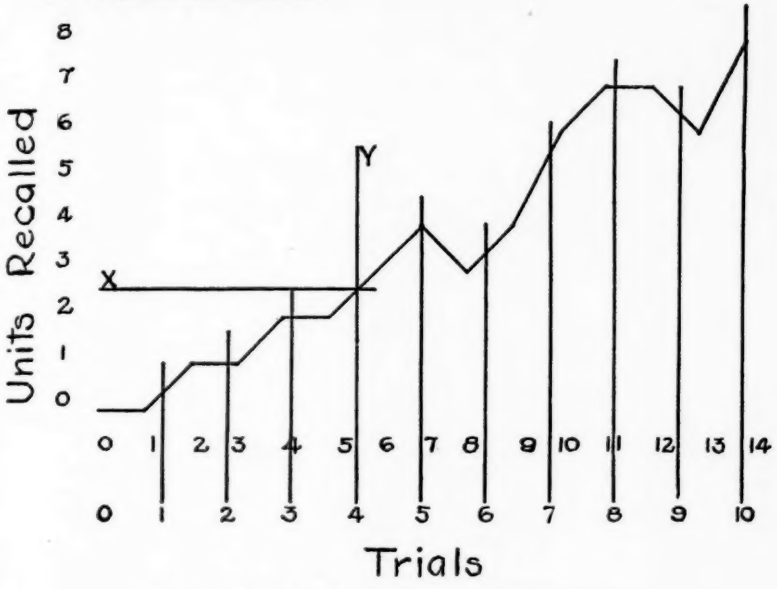

FIG. 1. METHOD OF EQUATING CURVES AS TO LENGTH

The material was presented visually by means of a memory apparatus, and the *Ss* read the words aloud. The following time-intervals were used for the different sets of materials: Set *a*, one word, 3.0 sec.; Set *b*, two words, 3.5 sec.; Set *c*, three words, 4.0 sec.; and Set *d*, four words, 4.5 sec.

The writer served as *E* in all experiments but one which will be mentioned later. The *Ss* were college students of undergraduate standing and practically all naïve in psychological experimentation.

Results. Table I and Figs. 2 and 3 show the results of Experiment I. Table I contains the data from which Figs. 2 and 3 were constructed. It gives the average number of correct responses for every decile, for every set of learning materials, under both experimental conditions. Fig. 2 gives the results for Condition A, and Fig. 3 for Condition B. The curves marked *a*, *b*, *c*, and *d* represent respectively the four sets of learning material.

TABLE I

AVERAGE NUMBER OF UNITS RECALLED PER TRIAL

		Decile										No.
	Set	1	2	3	4	5	6	7	8	9	10	*Ss*
Condition A	*a*	2.1	4.2	5.4	6.6	7.6	8.4	9.3	10.1	11.1	12.0	16
	b	1.2	3.7	5.1	6.1	7.3	8.0	9.0	9.6	10.4	12.0	18
	c	1.0	1.6	2.9	3.6	4.9	6.9	8.3	10.2	10.7	12.0	10
	d	0.4	1.2	1.8	3.2	3.3	5.1	6.8	8.8	10.0	12.0	10
Condition B	*a*	1.8	3.8	4.9	5.9	7.1	7.7	8.8	9.5	10.5	12.0	16
	b	0.7	1.9	3.2	4.5	6.4	7.6	8.6	9.9	10.9	12.0	18
	c	0.2	1.0	2.1	3.3	4.3	6.0	7.4	9.3	10.6	12.0	10
	d	0.2	0.6	1.5	2.6	4.0	5.1	6.8	8.3	10.1	12.0	12

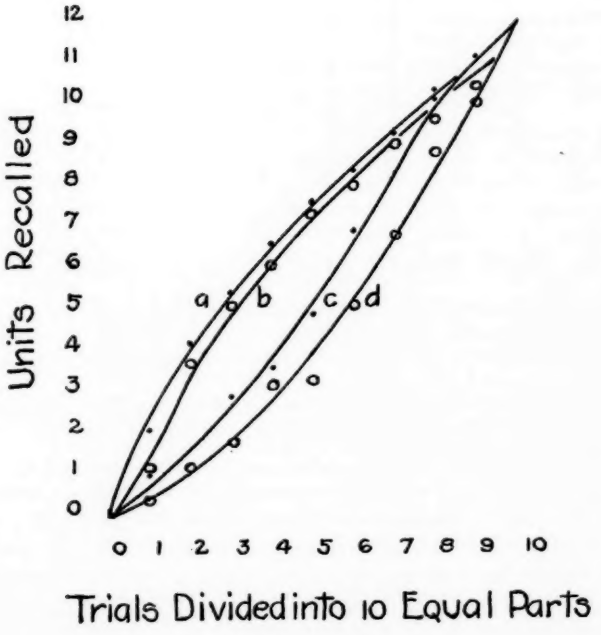

FIG. 2. LEARNING CURVES FOR MONOSYLLABIC NOUNS LEARNED IN ANY ORDER

These data show that both of the factors studied, viz. serial order and size or complexity of unit of response, are capable of contributing to the formation of s-shaped curves. While it is evident that serial order alone will not give s-shaped curves, inspection of the data reveals, that it is, however, an important factor. Curve *a*, Fig. 2 has less negative acceleration than Curve *a*, Fig. 3. Curve *b*, Fig 3 is

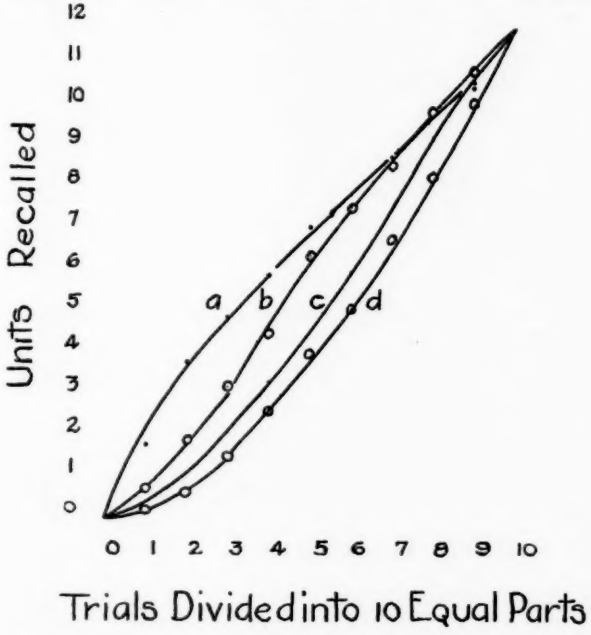

FIG. 3. LEARNING CURVES FOR MONOSYLLABIC NOUNS LEARNED IN SERIAL ORDER

decidedly s-shaped, while *b*, Fig. 2, representing material of the same complexity, is for the most part negatively accelerated, with possibly a slight initial positive acceleration.

Of the two factors studied, size of the unit of response seems to be the more important. Where the material is learned in serial order an initial positive acceleration is obtained when the size of the unit of response is increased to two words. This feature becomes more pronounced with further increase in the size of the units. Where the

material is learned in any order initial positive acceleration is obtained when the unit of response is increased to three and four words.

Summary. The results of this experiment may be summed up as follows. Negatively accelerated curves were obtained when the unit of response was a single word. With an increase in the size of the unit of response the curves assumed an initial positive acceleration with a subsequent negative acceleration. The inflection points on these s-shaped curves rise, with further increase in the size of the unit of response, until they vanish, as in the case of Curve *d* and leave positively accelerated curves. The appearance of the s-shaped curves is hastened by the factor of serial order.

The effect of complexity of material upon the form of the learning curve seemed sufficiently important to merit further study. Inasmuch as the methods used above permitted of no experimental control over the responses, save that contained in the instructions, it seemed desirable to adopt some other method. Hence the method was changed to that of anticipation, and a second experiment undertaken.

Experiment II

Apparatus and procedure. The material used in this experiment was similar to that used in the preceding experiment. The number of words and their arrangement into sets were also similar. Thus four sets of learning materials were used consisting of 12, 24, 36, and 48 words taken 1, 2, 3, and 4 at a time, respectively. The method and rate of presentation were the same as that used in Experiment I. The time intervals allowed for the responses were 4, 5, 6, and 7 sec., respectively, for the shortest to the largest units used. Presentation and recall were alternated.

The method was carefully explained and demonstrated to every *S* before the experiment began.[5] The *S*s were undergraduate students of psychology. Several had served in Experiment I, but the majority of them were naïve in so far as they had never served in psychological experiments.

The 12 and 24 word sets (Sets *a* and *b*) were usually learned by *S* at a single sitting, counterbalanced order being used for the different *S*s. The words of the other two sets (Sets *c* and *d*) were learned at one sitting, but by different *S*s. As before when the units of response consisted of two or more words, the *S*s were required to learn the words of such units in the order in which they were presented.

Results. Table II and Fig. 4 give the results for Experiment II. Fig. 4 shows the curves obtained by the method of anticipation for the four sets of learning material.

The curves obtained by the method of anticipation are in general agreement with those obtained by the method of direct recall. The

[5]The writer wishes to thank Miss Jean Clark for assistance in performing the experiments of this group.

TABLE II

AVERAGE NUMBER OF UNITS ANTICIPATED PER TRIAL

	Deciles										No.
Set	1	2	3	4	5	6	7	8	9	10	Ss
a	1.1	2.7	4.5	5.6	7.6	8.8	9.4	10.1	10.9	12	17
b	.4	1.9	3.3	5.3	6.6	7.6	8.7	9.6	10.7	12	16
c	.03	0.6	1.9	3.1	4.7	5.6	7.9	9.0	10.5	12	15
d	.1	0.5	1.4	2.5	4.5	5.6	6.9	8.7	9.9	12	12

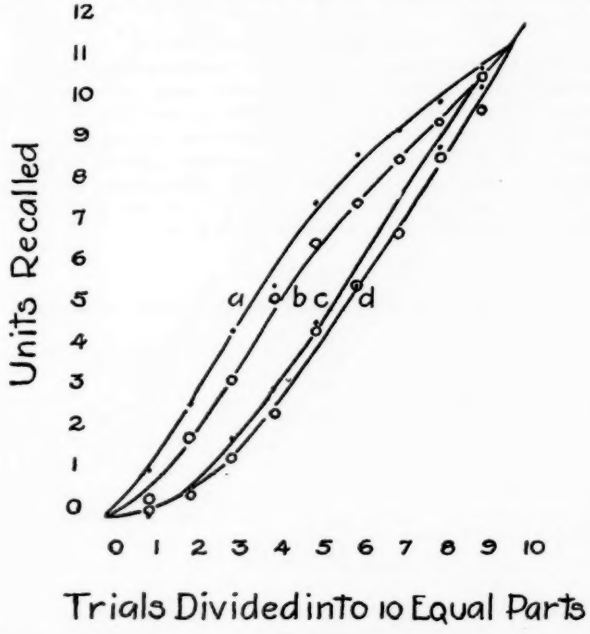

FIG. 4. LEARNING CURVES FOR WORDS BY METHOD OF ANTICIPATION

only notable exception is that of curve *a* Fig. 4, which is s-shaped, the point of inflection being between deciles two and three. Material of the same degree of complexity learned by the method of direct recall gave negatively accelerated curves. This is not a novel result, Kjerstad obtained similar curves for paired nonsense syllables and for paired monosyllabic nouns.[6] Such curves appear to be unstable

[6] *Op. cit.*, 82.

and subject to wide variation due to individual differences. Altus, cited below, using single lists of monosyllabic nouns varying in length from 6-30, attained composite curves of negative acceleration throughout.

The results of this experiment show that the form of the learning curve is significantly affected by the complexity of the unit of response. The inflection points rise to the vanishing point with increasing complexity of material.

Supplementary Experiments

(*1*) *Effect of exposure interval.* It seemed possible that the length of exposure period might affect the form of learning curves for complex material, although Kjerstad found that varying the interval from 1.5-3 sec. did not influence such curves for single columns of non-sense syllables and monosyllabic nouns.[7] An experiment was undertaken with the view to throwing light upon this question. Twenty-seven *Ss* learned three different lists of 24 words taken 3 at a time under three different conditions of exposure; namely, 3, 5, and 7 sec., respectively. Practice effect was controlled by counterbalanced order.

TABLE III

Effect of Exposure Interval upon Rate of Learning

	Deciles									
Interval	1	2	3	5	5	6	7	8	9	10
3 sec.	1.31	2.96	5.55	8.20	11.20	14.30	17.11	19.72	21.50	24.0
5 sec.	1.43	3.50	6.12	9.20	12.81	15.13	17.70	19.70	21.52	24.0
7 sec.	1.46	3.25	5.97	9.19	12.49	14.62	17.80	20.18	21.96	24.0

The results are given in Table III. It is readily seen that the form of the curves, platted from these data, would not be materially affected by these changes in time of exposure. This variation in interval, however, did affect the number of trials required to learn the lists. The average number of trials required to learn the three lists with the 3, 5, and 7 sec. intervals was 12.92, 12.08, and 10.79, respectively. It is interesting to note that the poorer learners benefited considerably by the longer exposure intervals while the better learners were practically unaffected by the changes in the length of interval. The average number of trials required by the poorest 14 *Ss* to learn the lists under the three conditions was 16.58, 15.17, and 12.4, while the corresponding scores of the best 13 *Ss* was 9.27, 9.00 and 9.18 trials.

(*2*) *Effect of practice.* It is possible to make some observation of the effect of practice on the form of learning curves from the foregoing data by comparing the rate of learning of the *Ss* on the three different days. The *Ss* benefited noticeably from the practice as shown by the reduction in the number of trials required to learn the material. The average number of trials required on the 1st, 2nd, and

[7] *Op. cit.*, 69-71.

3rd days was 14.4, 11.95, and 9.4, respectively. The effect of interval of exposure was controlled by a counterbalanced order. Table IV shows the average number of words recalled at each 10th of the learning period on the three different days. Inspection of this table reveals that the rate of progress was not affected by the amount of practice achieved in this experiment.

TABLE IV

EFFECT OF PRACTICE UPON RATE OF LEARNING

	Deciles									
Day	1	2	3	4	5	6	7	8	9	10
1	1.43	3.17	5.53	7.91	12.04	14.10	16.36	19.40	21.94	24.0
2	1.05	2.68	5.40	8.50	11.26	13.88	17.60	19.70	21.10	24.0
3	1.70	3.87	6.70	10.03	13.20	16.02	18.50	20.30	21.92	24.0

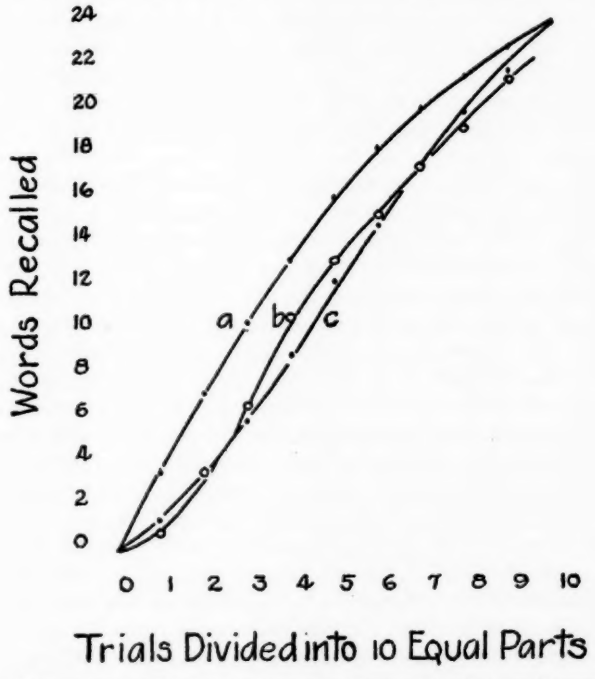

FIG. 5. CURVES FOR 24 WORDS ARRANGED IN THREE DEGREES OF COMPLEXITY

(3) *Complexity vs. length of material.* The question may arise as to whether the form of curves attained from the complex *sets* are due to complexity *per se* or merely due to the number of words used in these sets. The curves of Fig. 5 will

help to answer this question. Curve *a* is a composite of single curves of 20 *Ss* learning 24 monosyllabic nouns taken one at a time.[8] The method of anticipation was used. Curve *b* is taken from Fig. 4, Curve *b*, above and represents the rate of learning 24 words taken 2 at a time. Curve *c* is taken from the data in Table IV above. It is a composite of 3 curves each for 24 *Ss*. It thus represents the rate of learning 24 words taken 3 at a time. Here we have the same number of words learned by the same method, but varying in complexity.

These curves indicate that the changes in the form of the learning curves noted above are mainly due to the factor of complexity rather than to the varying number of words used.

Discussion

The results of the experiments indicate rather clearly that the form of learning curves is materially affected by the size, or complexity of the unit of response and to a lesser degree by the experimental method employed. On the basis of the experimental evidence it appears that the s-shaped curve may be regarded as an intermediate curve between negatively and positively accelerated curves, the type of curve obtained depending upon the complexity of the unit of response.

The results seem to warrant the following general statements. (1) As the complexity of material of a given length is increased the learning curves will change from negative toward positive acceleration; the intermediate curves will be s-shaped. (2) As the complexity of material is increased the inflection points on the intermediate s-shaped curves will rise, approaching the vanishing point. (3) The greater the complexity of a given bit of learning material the higher will be the threshold of 'recallability' of the respective units and the greater will be the number of readings required to reproduce them. (4) When material of a given length is read as a whole with somewhat equal distribution of attention to all of the units, the relative rapidity with which the successive units are recalled by a given subject will be proportional to the number of readings required.

The first and second generalizations may be regarded as descriptive, while the third and fourth may be regarded as explanatory.

The distribution of attention is considered an important matter in experiments of this kind. If no experimental control is exercised in this respect *Ss* may differ sufficiently in direction of attention to alter the form of the curves. Somewhat equal distribution of attention is regarded as most favorable for s-shaped curves and those of positive acceleration. If an *S* adopts a method grasping a definite number of

[8]The writer is indebted to Mr. W. D. Altus for these data.

units per trial the learning curve will tend toward a straight line, provided the rate can be maintained. Some approach toward equal distribution of attention may be achieved by having the *Ss* read the material aloud. This may be supplemented with instruction.

Conclusions

(1) Types of learning curves obtained for ideational material depend upon complexity of the material. (a) By the method of direct recall negatively accelerated curves were obtained for the 1-word units; s-shaped curves were obtained for the 2- and 3-word units; positively accelerated curves were obtained for the 4-word units. (b) By the method of anticipation s-shaped curves were obtained for the 1-, 2-, and 3-word units, and a positively accelerated curve was obtained for the 4-word units.

(2) The inflection points on the s-shaped curves rise with increase in size of the units of response.

A QUANTITATIVE STUDY OF THE RELATION BETWEEN PULSE AND BREATHING CHANGES AND ELECTRO-BIOCHEMICAL RESPONSES

By Alvhh R. Lauer, Iowa State College, and Erma A. Smith, University of Chicago

So much has been said relative to bio-electrical phenomena within recent years that further elaboration is unnecessary here. Landis and Dewick have very thoroughly reviewed the literature.[1] It suffices to say that the variation in methods of procedure would alone account for certain discrepancies in results noted. However, the immense amount of time and effort spent upon this type of investigation would tend to indicate the existence of a certain basic phenomenon which so far has not been satisfactorily measured.

It is our contention that various factors are involved in electrical changes of the living organism, and the present study is an attempt to determine the association of such relationships. Particular attention is given to the temporal sequences of different processes selected for measurement.

The junior author has shown that electrical accompaniments of certain responses can be measured consistently if the physical characteristics of the apparatus are held constant.[2] When a measure can be repeated with a certain assurance of securing consistent results, the technique may be used to determine, if possible, the antecedent-consequent relationships, or associations of such phenomena as may be under consideration. The following study was thus undertaken to discover the relation of certain physiological processes occurring immediately after sensory excitation.

Method and Apparatus

The laboratory method is here supplemented by statistical analysis. Measurements were made of 30 college sophomores making a total of 133 reactions. The averages of each were used for tabulating data on the various stimuli. For correlation purposes the 133 reactions were treated separately.

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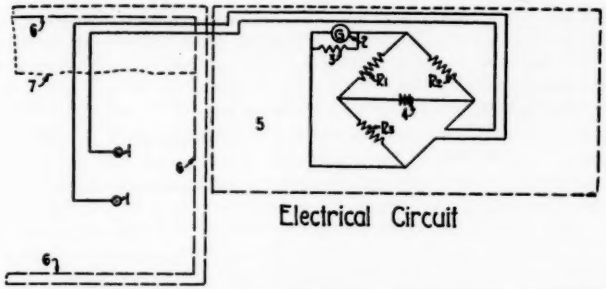
[1] C. Landis and H. N. Dewick, The electrical phenomena of the skin (psycho-galvanic reflex), *Psychol. Bull.*, 26, 1929, 64-119.

[2] A. R. Lauer, Reliability of the psycho-galvanic reflex, this Journal, 41, 1929, 263-270.



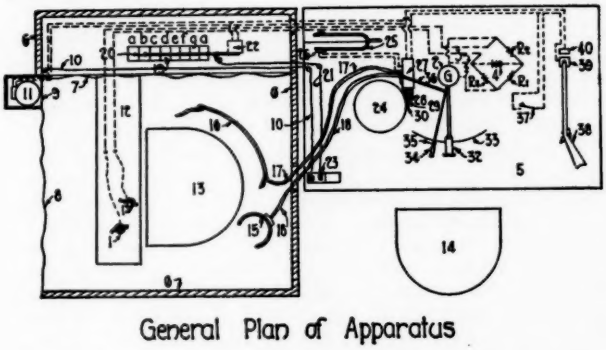


FIG. 1. SHOWING THE WIRING CIRCUIT AND THE GENERAL PLAN OF THE APPARATUS

(1) Electrodes
(2) Galvanometer
(3) Shunt
(4) 3-volt battery
(5) Table
(6) Semi-darkened booth
(7) Screen shielding the xylophone and hammer
(8) Curtain shielding skull
(9) Skull support
(10) Cord for releasing and lighting skull
(11) Skull
(12) Support for *S*'s hands
(13) Chair
(14) Chair for E
(15) Sphygmograph
(16) Pneumograph
(17) Connecting tube for 16
(18) Connecting tube for 15
(19) Xylophone
(20) Sound hammer
(21) Hammer release cord
(22) Contact for registering hammer stimulus
(23) Contacts for registering release of all stimuli
(24) Kymograph
(25) 6.57 d.v. fork for timing
(26) Contact on fork
(27) Signal magnet
(28-30) Signal magnets
(32) Telescope
(33) Telescope scale
(34) Needle pointer
(35) Lever for tracing galvanic curve
(36) Controls for curve marker
(37) Key for incidental stimuli
(38) Rifle
(39) Bracket for stimulus-contact
(40) Contact to register firing of gun

The *S* was seated in a cushioned chair (13) (see Fig. 1) within a darkened booth and instructed to sit quietly and relax. Information was further given

that surprise stimuli would be presented. A Marey tambour (15) was attached to the left side of the neck just over the external carotid artery and fastened by means of a special strap around the neck. This tambour was in turn connected with the sphygmograph which recorded on a smoked drum (24). A pneumograph (16) was also attached to *S* in the same general way.

The electrical changes were measured by a Wheatstone bridge (4) with a d'Arsonval galvanometer of the Leeds and Northrup fabrication, having 500 ohms coil resistance, 300 megohms sensitivity, with a 3-sec. period and critically damped. The regular Féré technique was used with a 10:1 ratio in the two arms of the bridge and a 3-ohm shunt across the terminals of the galvanometer. The junior author's liquid electrodes were employed,[3] being attached to the palms of the hands. A 3-molar NaCl solution was used as the electrolyte. The two hands were placed on a cushion supported by a long desk (12).

By means of a needle pointer (34) attached to the lever (35) the deflections were registered directly on the kymograph drum (24). Thus the pulse, breathing, and electrical changes were recorded simultaneously, as shown in the sample records in Fig. 2. Time was marked by a signal magnet (29) connected in series with a 6.57 d.v. fork. The fork was calibrated for this experiment.

In the same way the administration of stimuli was registered by a signal magnet (30) in series with a multiple arc circuit, each branch of which respectively was closed with the presentation of one type of stimulus. The latter consisted of: (a) a small xylophone bar (19) struck by a sound hammer (20) raised to the height of 30 cm. and allowed to fall of its own momentum; (b) a skull (11), hidden behind the curtain (8), which was illuminated from within by a 60-w. bulb when the cord (10) pulled aside this covering; (c) the shrill blast of a policeman's whistle blown by *E*; and (d), a shot from a .22 short blank cartridge fired from a clamped rifle (38) pointed in the opposite direction from *S*. The policeman's whistle was less constant than the other three types of stimuli, but in any case the element of personal proximity to *S* was excluded.

After the *S* was seated for a few minutes and his resistance stabilized, the first stimulus was given. The measurement was made, and after stabilization the bridge was rebalanced, if necessary, and a second stimulus given. In about 70.1% of the cases no rebalancing was necessary. The stimuli were generally given in the following order: sound of hammer, whistle, skull, and rifle. In a few cases the order was changed and usually the stimuli were repeated. Mean values were used as scores in the latter cases.

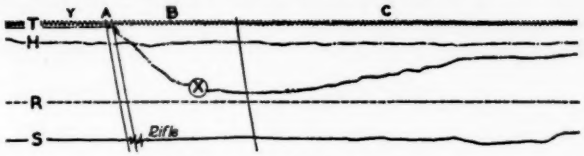
The records furnished several kinds of data for analysis. Body resistance values were copied directly on the data sheet from the bridge reading. Using a method of calibration previously described by the junior author,[4] the resistance and deflections were used to compute the actual ohms change of the *S* and the percentage change. The latter is a result of the actual change in ohms divided by the initial body resistance.

Considering Fig. 2, Curve 1, it will be seen that the galvanometer deflections may be divided into three separate phases with respect to temporal sequence.

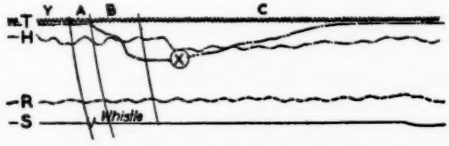
[3]Lauer, A new type of electrode for the galvanic skin reflex, *J. Exper. Psychol.*, 11, 1928, 248-251.

[4]Lauer, *op. cit.*, this JOURNAL, 263-270.

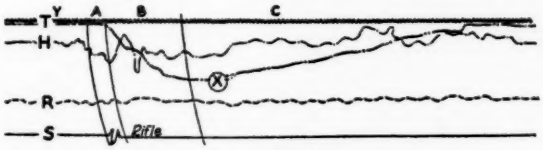
There is, first, the period of time after a stimulus is given until the deflection takes place (A); second, the time required for the deflection to reach a maximum (B); and third, the time required for the return to normal (C).[5]



Curve No. 1 R.B. at 4.15 P.M.



Curve No. 2 A.E. at 5.00 P.M.



Curve No. 3

FIG. 2. SAMPLE RECORDS OF THE PULSE, BREATHING, AND ELECTRICAL CHANGES

T = time line in 1/6 sec.; H = pulse; R = respiration; S = stimulus; X = galvanometer; Y = stable period before stimulus; A = time between stimulus and beginning of response; B = time required for maximum galvanic response; C = time required for galvanometer to return to a point of stability.

In Curve No. 1 the galvanometer did not return to normal. Records like this were obtained in about 30% of the cases. Another unusual characteristic of this curve is the very short duration of period A. Curve No. 2 is more typical of the results obtained in this study. In Curve No. 3 the pulse line (H) shows marked changes—the startling effect of the rifle shot.

In addition to these phases of the electro-organic response, pulse rate and breathing curves were recorded. The curves of Fig. 2 show these results in detail. In addition, body resistance could be found from the data sheet upon

[5]It may be well to say that there was no clean-cut case of the resistance's having increased immediately after a stimulus. There is always a possibility that attaching the electrodes may produce ohmic changes, and in certain *S*s this may persist for an unusually long time. In one instance, after the bridge was balanced there seemed to be a gradual increase in resistance. After stabilization the stimulus was presented and the resistance continued to increase. Later excitations of the same *S* failed to produce the same results.

which a limited protocol was made. The records were read by means of a small glass in units of slightly under 1/6 of a second as given above. Measurements of segments of the curves were made in millimeters from hairline scratches made on the fixed records. These data of 133 reactions from 30 *Ss* were punched on Hollerith cards, and part of the results computed from the latter. In order to study the nature of the relationships the correlations were plotted on standard sheets.

Results

Table I shows that (1) the measures are consistently in one direction which is an indication of the significance of measurements, and (2) there is a fair correspondence in magnitude of the responses with the intensity of the stimulus.

TABLE I

Extent and Direction of Reactions to Different Stimuli

	Bell	Skull	Whistle	Rifle	Mean
Breathing (percentage).....	− 1.6	− 3.0	− 2.7	− 5.3	−1.80
Heart rate (percentage).....	+ 8.4	+ 7.9	+ 6.6	+ 9.6	+8.12
Galvanometer deflections in cm.....	+ 7.8	+ 7.6	+ 6.5	+11.5	+9.09
Change in ohms resistance..	−1560	−2240	−1104	−2426	−1845
Percentage ohms change....	− 7.8	− 8.7	− 6.7	−10.9	−8.20

In Table II the following facts seem significant: (1) the first response is delayed longer than those made to subsequent reactions; (2) the greater the response the longer is the period before return to normal; (3) there is very little difference between the various stimuli with respect to the time between administration of the stimulus and the point of maximum deflection.

TABLE II

Duration of Different Periods of Galvanometer Deflection

Time (in sec.)	Bell	Skull	Whistle	Rifle	Mean
From stimulus to response...	4.39	3.76	3.81	3.77	3.94
For deflection to reach maximum.....	10.39	11.64	9.19	11.76	10.78
Sum.....	14.78	15.40	14.00	15.53	14.72
For galvanometer to return..	21.75	24.64	23.55	39.52	27.03

The temporal relation of these changes suggested a further comparison with existing data. According to Buchanan,[6] pulse changes taking place immediately following the onset of physical exercise,

[6] F. Buchanan, The physiological significance of pulse rate, *Trans. Oxford Scientific Club*, 14, 1909, 351.

increased 31.8% the first ½ sec. Within 4 sec. after a sensory stimulus was given in our experiments the increase was only 5.1%. The total increase over the resting rate was 9.1%. The upper half of *S*s (those having greatest heart increase) were sorted out by the tabulating machines and compared with respect to time and magnitude of deflections. This is shown in Fig. 3.

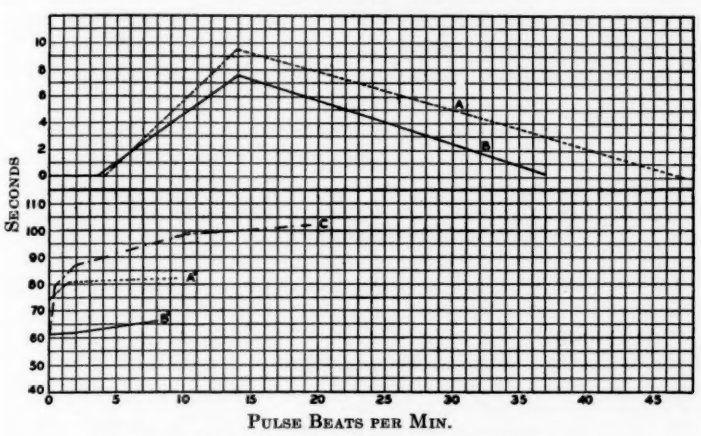


Fig. 3. Curves Showing Changes in the Galvanic Response and Pulse Rate

Curves A and B represent the galvanic response periods for the upper and lower 50% groups divided on the basis of pulse rate. Curves A′ and B′ show the relative amounts and rate of pulse changes for the same groups. Curve C shows Buchanan's increase of pulse rate with exercise.

It is evident from the curves in Fig. 3 (a) that pulse changes take place much more rapidly than galvanometer deflections from biochemical responses; (b) that persons with a higher normal pulse rate tend to have greater proportional increases than do those with lower pulse rates; and (c) exercise increases pulse rate much more, but the time for the increase is about the same.

In addition to the measurements described above, the standing pulse rate was taken after *S* had rested 2 min. in the laboratory. He was then caused to walk 100 steps at a brisk rate. No graphic record could be made; only a record of pulse rates as taken from the radial artery before and after exercise. The percentage increase

with exercise was correlated with the percentage increase after sensory excitation. An $r = -0.2723 \pm .11$ was obtained, indicative of at least a zero, if not a slightly inverse relationship, between these two phenomena. This would seem to signify that different factors are involved in pulse increases during exercise and those during or following sensory excitation.

Finally the 133 reactions were considered separately and correlated to determine the various intercorrelations of the 7 variables used in the major series of this study. The results are given in Table III, which shows the following facts:

TABLE III

INTERCORRELATIONS OF CHANGES FOLLOWING SENSORY EXCITATION

Hc = heart change in beats
Td = time for maximum deflection
Oc = actual ohms change
Dc = deflections in cm.
Tr = heart change in beats
Rt = time of galvanometer return
Br = initial body resistance
Bc = breathing changes in percentage

(The probable errors range from ±0.060 to ±0.022)

	Hc	Td	Oc	Dc	Tr	Rt	Br
Td	+.7962						
Oc	+.3813	+.5478					
Dc	+.3254	+.4178	+.1664				
Tr	−.0754	+.0099	−.0410	−.0736			
Rt	+.3216	+.2135	+.2366	+.4189	−.1177		
Br	+.1640	+.1902	+.6347	−.1568	+.1581	−.1367	
Bc	−.1892	+.0100	+.0157	+.0877	−.0845	+.0122	−.0084

(1) There are significant associations between the following variables: (a) heart changes and the time required for the deflection to reach a maximum; (b) initial resistance and ohms change; (c) ohms change and heart variation; (d) magnitude of deflections and heart variation; (e) return time and pulse change; and (f) possibly between pulse and breathing changes.

(2) There is no significant relation between breathing changes and any of the six other variables studied. The reliability of the measurements of breathing changes was not determined, but the consistency of changes shown in Table I tend to support these results as worth consideration.

(3) Other apparent relationships between return time, deflections, ohms change, and initial resistance are inherent, due to characteristics of the apparatus as described by the junior author.[7]

[7] Lauer, *op. cit.*, *J. Exper. Psychol.*, 11, 1928, 248-251.

A further study of the relation between body responses and electrical changes was made by multiple correlation technique. The formula, $R_{c \cdot 12} = [(r^2_{c1} + r^2_{c2} - 2r_{c1}.r_{c2}.r_{12})/ (1 - r^2_{12})]^{\frac{1}{2}}$ was used. In turn each aspect of the galvanometric curve and its characteristics was used as a criterion with the heart and breathing changes set as independent variables. The results shown in Table IV were obtained.

TABLE IV

MULTIPLE CORRELATION COEFFICIENTS

$R_{Rt.Bc.Hc.} = +.3301$	$R_{Oc.Bc.Hc.} = +.4177$
$R_{Tr.Bc.Hc.} = +.1038$	$R_{Td.Bc.Hc.} = +.8131$
$R_{Dc.Bc.Hc.} = +.3264$	$R_{Br.Bc.Hc.} = +.1655$

From Table IV it is obvious that the length of time required for the electrical changes to take place is most closely associated with pulse and breathing changes. The second closest asociation is the actual ohms change; next comes the time for return to normal, and finally the amount of deflection. Of course some of the other measures may be more important, depending upon the criterion used, but the facts of association are clearly shown.

SUMMARY

(1) When external conditions are held constant the magnitude of the electro-biochemical response is roughly proportional to the strength of the stimulus.

(2) Changes in pulse rate from exercise seem to be brought about by different factors than those producing changes in pulse rate after startling or exciting stimuli.

(3) The period of time required for the galvanometer to return to normal seems to be important, as measuring responses different from those involved in sheer physiological changes.

(4) Body resistance seems only slightly correlated with susceptibility to change after sensory excitation.

(5) Changes in pulse rate after the stimuli were given are highly correlated with the speed of the response.

(6) Breathing changes seem to have no correlation with the electrical changes measured by this technique.

THE RELATION OF BRIGHT AND DULL PRESSURES TO AFFECTIVITY AND THE METHOD OF CORRELATION

By Elizabeth Converse, Bryn Mawr College

In an endeavor to settle the issues raised by Young's criticism[1] of Nafe's reduction[2] of pleasantness and unpleasantness to bright and dull pressures, Hunt proposed the method of correlation as a solution.[3] Young claimed that Nafe's results were due to laboratory atmosphere. While Nafe denied this, he did admit that bright and dull pressures were not 'attention demanding.' In order to find them the *O*s must be set for them. In order to avoid laboratory bias, the *O*s must not be told of any connection between these pressures and affectivity. Hunt, therefore, instructed his *O*s to report at one time upon the bright and dull pressures aroused by visual stimuli, and at another time upon pleasantness and unpleasantness. "The correlation between the two sets of reports would then support Nafe's claims or disprove them."[4] Experiments with different groups of *O*s and under a variety of checks gave consistently high correlations between these two sets of qualities, with sufficiently small probable errors to insure statistical reliability. Hunt's results are obviously not due to chance. Of three possible interpretations, Hunt concludes that "affection is accompanied by bright and dull pressures," rather than "affection *is* bright and dull pressure" (Nafe's view), or that these qualities depend upon identical receptor processes but different attitudes of report (the third view).[5]

Experiment I

Following the suggestion made by Hunt that further investigation is necessary to determine which of these views is correct, and, we may add, to determine if some other view may not be even more

*Accepted for publication July 9, 1932. This study was directed by Professor Harry Helson.

[1] P. T. Young, Studies in affective psychology, this Journal, 38, 1927, 157-193.

[2] J. P. Nafe, An experimental study of the affective qualities, *ibid.*, 35, 1924, 507-544.

[3] W. A. Hunt, The relation of bright and dull pressure to affectivity, *ibid.*, 43, 1931, 87-92.

[4] Hunt, *ibid.*, 88.

[5] *Ibid.*, 92.

plausible in the light of added evidence, we decided to repeat Hunt's work, keeping conditions, during part of our experimentation, identical with his.

Procedure. In the first set of experiments Hunt's procedure was followed as closely as possible in every respect. Visual stimuli were used, as in his experiments, in order to avoid local pressures. They consisted of 20 standard Milton-Bradley colored papers of various hues, brightnesses, and saturations. The stimuli were 5 in. sq. and exposed behind the opening in a large gray cardboard screen. The *Os* were 4 ft. from the screen, eyes level with the stimulus. Palo daylight lamps served as the source of light in a dark room in order to insure constant illumination throughout. Fifteen persons acted as *Os* for this set of experiments, their psychological training being as follows: 4 *Os*, (*G*, *Gr*, *H*, *Y*) were seniors and somewhat experienced in observing as they were conducting experiments of their own and observing in those of each other; 10 *Os*, (*B*, *C*, *K*, *Ch*, *Br*, *Hu*, *Ba*, *Ka*, *P*, and *Q*) had had only an elementary course in experimental psychology, and 1 *O*, (*M*) was then taking her first course in psychology but had not then arrived at the laboratory work.

The instructions resembled Hunt's and were as follows for *G*, *Gr*, *H*, and *Y*:

I shall present you with various colored stimuli and you are to report whether the experience involved is pleasant, unpleasant or indifferent.

The stimuli were given in constant order 10 times: 5 at the beginning of the week and 5 at the end. Every color was exposed for 5 secs., with a 10-sec. interval between successive presentations during which the *Os* gave their report. The following week the same colors were given again in the same way, but this time the instructions called for reports on bright and dull pressure. The instructions and explanatory comments used by Hunt were followed.

I shall present various colors. You are to attend to the general experience involved in observing them, and to state whether this experience involves a bright pressure or a dull pressure. These pressures are qualitatively similar to those ordinarily elicited by cutaneous stimulation, but in no way identical with them.

You are to report whether the color you have seen is accompanied by bright pressure, dull pressure, or nothing in the way of pressure.

The explanation of terms consisted of the following comments:[6]

'Bright pressure' has been described as a tactual bright pressure or touch quality lying between bright pressure and tickle. It has expansive qualities and has been called radial, being more prominent at the edges than in the center. It is bright, buoyant, and has a diffuse bodily feel. The whole experience is diffuse and volumic, and is patterned with the patterns varying from one experience to another. The following adjectives have been used in describing it: bright, sparkling, brilliant, active, effervescent, like tickle, dancing, shimmering, misty, mild, vaporous, light, airy, fluffy, ethereal, smooth, soft, welling, spreading, like expansiveness of the body.

'Dull pressure' has been described as a dull pressure or a quality lying near dull pressure, or between dull pressure and strain. It is not extended. It has qualities of constriction and has been called radial, but more prominent at the center than at the edges. While it is diffuse and volumic, it is less so than bright pressure and is duller and less bright. It has been called dull, massive, drab, dead, rigid, less lively, somber, inert, stiff, gloomy, more dense, heavy, sinking, leaden, thick, cold, rough, grating, insistent, condensed, like bodily contraction.

[6]Hunt's instructions and explanatory comments were given verbatim, *ibid.*, 89.

Both the affective and pressure judgments were given numerical values as follows: pleasantness and bright pressure were given +1, indifferent 0, and unpleasantness and dull pressure −1. The coefficients of correlation were then calculated by the product-moment formula.

A second group of *Os* (*B, C, K, M, Q*) were given these same colors and the affective judgments called for first and the pressure judgments later. Only 3 series, or repetitions of each stimulus, were given this time as it was found that the judgments remained reasonably constant from the very first in the previous observations.

A third group of *Os* (*Ba, Br, Ch, Hu, Ka, P*) were given 3 series in which the pressure judgments were called for first and later three more series in which the affective judgments were called for. This group was to serve as a check on the other two so far as order of judgments went.

Results. The results of the experiments with the three groups of *O*s are given in Table I.

TABLE I

SHOWING, FOR EVERY *O* OF EVERY ONE OF THE THREE GROUPS, THE CORRELATION BETWEEN THE AFFECTIVE AND PRESSURE JUDGMENTS

Group I			Group II			Group III		
O	r	P.E.	*O*	r	P.E.	*O*	r	P.E.
G	.43	.15	*B*	.59	.09	*Ba*	.32	.14
Gr	.0		*C*	.51	.11	*Br*	.42	.12
H	.66	.09	*K*	.0		*Ch*	.28	.14
Y	.09	.11	*M*	.18	.15	*Hu*	.003	.15
Av. r = .30			*Q*	.50	.10	*Ka*	.82	.05
			Av. r = .36			*P*	.0	
						Av. r = .31		

In every one of the groups one *O* failed to report any pressures with a consequent correlation coefficient of zero. These zero values do not depress the average coefficient of correlation very much. We felt no reason existed, therefore, for discarding the results, particularly as the *O*s seemed to be trying to follow the instructions.

The striking result from our experiments is the low correlation between bright and dull pressures with pleasantness-unpleasantness, low as compared with Hunt's results. The average coefficient of correlation for Hunt's 11 *O*s is 0.89 while that for the 15 *O*s used in our experiments is 0.36. If Hunt's probable errors of his coefficients of correlation are to mean anything they should give some indication of what is to be expected from a random sampling of coefficients of correlation based upon the same conditions. The coefficients for our *O*s fall so far outside the limits of Hunt's as to leave no doubts concerning the significance of the difference between his and our results. So

far it would appear that a random sampling of *O*s does not result in a duplication of Hunt's high correlations even when his method of procedure has been followed to the letter—in so far as this is possible at another laboratory.

Experiment II

In order to favor Hunt's hypothesis we next used auditory stimuli which Hunt rejected because they might produce local pressures. This possible criticism of Nafe's work which Hunt wished to avoid in his impartial approach we decided to incur in the hope that better conditions for a high correlation would serve as a better test of the Nafe-Hunt theory.

Procedure. The stimuli in the first auditory experiments consisted of the eight tones of the 2-A audiometer, having frequencies of 64, 124, 256, 512, 1024, 2048, 4096, and 8192 cycles, given at a dial setting of 50 for each in order to keep the intensity of the tones constant and well above the threshold.

The *O*s in this part of the experiment were *C*, *G*, *Gr*, *H*, *Q*, and *Y* who had taken part in the previous experiments and in addition, two experienced *O*s, *F* and *Hi*, graduates in psychology. Fifty repetitions of every tone were given to every *O*. Every tone was given for 5 sec. and an interval of 10 sec. allowed for rest and report, and after every 7 repetitions a longer rest was allowed. The instructions were the same as above, except for the substitution of the word 'tones' for 'colors.' The first 4 *O*s were asked to give judgments of pressures first, and a week later judgments of pleasantness-unpleasantness, while the judgment reports were reversed in the case of the last named *O*s. The results were treated in the same way as described above for the color judgments.

Results. The results of Experiment II are given in Table II, which shows the coefficients of correlation, obtained from the *O*s of the two groups, between the affective and pressure judgments. All

TABLE II

Showing, for Every *O* of Each of the Two Groups, the Correlation between the Affective and the Pressure Judgments

Group I			Group II		
O	r	P.E.	*O*	r	P.E.
G	−.72	.20	*C*	.89	.05
Gr	.33	.21	*F*	.0	
H	.36	.21	*Hi*	.69	.13
Y	−.98	.001	*Q*	.0	
Av. r = −.25			Av. r = .39		

the *O*s of Group I reported pressures, including one *O* (*Gr*), who did not in Experiment I. The *O*s of Group II gave results very different from those obtained from Group I.

The results with the first set of *O*s on the tones gave an average correlation of −0.25, while the second gave an average correlation of +0.39. If we average both groups we get a coefficient of +0.07, which is nearly zero correlation. Analysis of the individual results shows that anything may happen. The two negative correlations are practically statistically reliable, as well as being fairly high while the same may be said for the two positive correlations in the second group. Two correlations are zero, even though pressures were reported, and two are neither sufficiently high nor sufficiently above their probable errors to be significant. Hunt, in discussing his coefficients, says, "if the effect of variability . . . could be cancelled out, the correlations would approach a value of +1.00 or perfection." As a matter of fact, a larger universe, as represented by the addition of our *O*s, gives lower correlations and contradictory results for different *O*s. Since Hunt's results all were in one direction and consistently high we must conclude that there existed some unspecified conditions favoring high correlations between the affects and pressures in his experiments. The results from our experiments, low, high, negative, and zero correlations, argue much more convincingly for 'neutral' conditions than do the results of Hunt.

Experiment III

Owing to the fact that some *O*s objected to the tone stimuli on the ground that they were devoid of feeling-quality, another set of experiments was designed in which auditory stimuli were presented having affective quality.

Procedure. Ten musical records containing selections from classical music, jazz, and 'strange music' were used.[7] Only the first few opening chords or bars were used containing a simple theme or melody and lasting only several seconds. They were presented three times in succession and the *O*s were asked to give their judgment on the last presentation. Pleasant-unpleasant judgments were given first, and then bright- and dull-pressure judgments.

Results. The results of Experiment III are shown in Table III. Despite the fact that we used in this experiment stimuli that had very definite affective values, which were much richer than the colors and pure tones used in the preceding experiments, the correlations were

[7]The following records were used: Brahms, Symphony 1, Movement 1, *Victor, 6660-A*; Selections from Traviata, *Victor, 35807-A*; You Rascal, You, *Brunswick, 6225*; Debussy, Afternoon of a Faun, *Victor, 6696-A*; Brahms, Symphony 1, Movement 3, *Victor, 6660-B*; Rimsky-Korsakoff, Scheherezade, Movement 1, *Victor, 6738-A*; I Don't Know Why, *Columbia, 2540-D*; Selections from Traviata, *Victor, 35807-B*; Coplas del Terruno, *Brunswick-Bogata, 40267-A*; Return of Peer Gynt, *Victor, 9328-A*.

not significantly higher than before. With one exception, neither the correlations themselves, nor considered with their probable errors are significant.

TABLE III

SHOWING, FOR EVERY *O*, THE CORRELATION BETWEEN THE AFFECTIVE AND PRESSURE JUDGMENTS

O	r	P.E.
C	.86	.06
G	.31	.19
Gr	.0	
H	.23	.21
Y	−.10	.21

We must conclude, therefore, that the occasionally significant coefficients of correlation found in our results are but due to random sampling and that for our group anything may occur: there may be high, low, zero, and even negative correlations between bright and dull pressures and pleasantness and unpleasantness. A larger universe does not help Nafe's or Hunt's case. It must be remembered that our *O*s were not instructed either for or against the theory that pleasantness *is* or is accompanied by bright pressures and unpleasantness *is* or is accompanied by dull pressures.

But Hunt got consistently high correlations between these two sets of qualities. He believes they support a particular analysis made by Nafe, or the latter's *O*s, Before we take up the question of the meaning of correlation, whether high or low, let us report another set of experiments and their results.

EXPERIMENT IV

We suspected that if any pair of opposites taken to be mutually exclusive by our *O*s were correlated with another pair of similar terms (pleasantness and unpleasantness, for example), the chances were good that some correlation would be found. We chose the paired terms 'personal-impersonal.' If one were asked what these terms have to do with pleasantness-unpleasantness we must confess we do not know; nor do we know what bright and dull pressures have to do with pleasantness and unpleasantness. Hunt implies that he did not either, but held that if the method of correlation should yield high coefficients, then Nafe's contentions would be supported. In that case we would be justified in concluding from the results about to be reported that affection is either personal or impersonal, or that it is

accompanied by personal and impersonal (what?), or both depend upon identical processes but different attitudes.[8] Hunt's logic must apply to any pair of correlated variables if the correlations are sufficiently higher than their probable errors.

Procedure. Three series of the 20 colored stimuli previously used were given to *H*, *G*, *Gr*, and *Y*, with the following instructions:

I shall present you with various colored stimuli and you are to report whether they are personal or impersonal. Judge them according to your own definition of personal and impersonal.[9] Later you will be asked for your criteria of personal and impersonal. Try to keep as near as possible to the generally accepted meanings of these words.

We give some of the definitions of these words as reported by the *O*s.

Y: A color was personal if it was warm and rich and had a meaning, whether pleasant or unpleasant; and impersonal if indifferent and cold, with no meaning or effect.

H: Personal was a color intimately connected with myself, a color I might like to wear or have around, while impersonal was the opposite, being in no way connected with myself.

G: A personal color concerned definite things or a definite type of person; or a definite type of person might wear it. Impersonal colors, on the other hand, were general and might be associated with anything or anybody; anybody might wear them.

Gr: Colors were judged as personal or impersonal when they reminded me of something or of some person associated with me, whether pleasant or unpleasant. Impersonal colors called up no such associations.

Results. The results of Experiment IV are given in Table IV. The correlations shown here, as judged by statistical criteria, are all significant, two of them being high. If the reader will turn to the results for this group of *O*s in Experiment I he will find that they are much lower and with one exception do not possess reliability. But the correlations between affective value and 'personal-impersonal' are all fairly high and more than 4 times their probable error. Are we to conclude that all affective values are personal-impersonal or that the latter 'accompany' the former, or that they have identical receptor processes but different attitudes?

Having high correlations that are statistically reliable are we to conclude that the correlated variables can be reduced to one another, are associated with or accompany one another? Authorities in this field speak as follows: "Persons making use of the coefficient (of correlation) should be warned that it reveals conditions which, though

[8] These seem to be the only ways in which Hunt seems to be able to interpret a high correlation coefficient. We shall see that there are other interpretations which do not favor the Nafe reduction.

[9] Nafe's *O*s, the original discoverers of bright and dull pressures, must have used their own definitions of these terms so we were introducing nothing new from the point of view of psychological method.

necessary, are insufficient for inferences as to causal relations and common elements and other hypotheses involving extra-statistical consideration."[10] It would appear that Hunt's high correlations mean as little for the relationship between pressures and affects as ours do for the relationship between personal-impersonal and the affects. We are not concerned to argue the question as to how Nafe's results were obtained. We believe that Young has furnished a completely convincing explanation. We are only concerned here to show that it is possible to get high correlations between variables which

TABLE IV

SHOWING, FOR EVERY *O*, THE CORRELATION BETWEEN JUDGMENTS OF PERSONAL-IMPERSONAL AND PLEASANTNESS-UNPLEASANTNESS

O	r	P.E.
G	.54	.11
Gr	.57	.10
H	.87	.04
Y	.75	.07

have no psychological relationship to each other, other than that set by the conditions of the experiments. Ask a set of *O*s, with the suasion that goes with the atmosphere of an experiment, to judge a set of stimuli by means of two sets of terms, each containing antithetical terms, and we venture to predict that more often than not high correlations will be found from such procedure. Unless there is other ground for believing that some significant relationship exists between two variables, correlation furnishes no scientific basis for its assertion. High correlations may indeed lead us to look for more significant, psychological relationships but they do not and cannot settle the issues raised in the Young-Nafe controversy. On the contrary, high correlations may be due as much to laboratory atmosphere or other unspecified and unspecifiable conditions as introspective analyses.

The chief test by which charges of laboratory bias can be met is that of reproducibility of results found in one laboratory by another laboratory. We have failed to reproduce Hunt's high correlations for pressures and affectivity just as Young failed to reproduce Nafe's analyses of the affects into pressures. But even had we been able to get the high correlations Hunt found, we have shown that these can by no means be interpreted in the manner Hunt would have them.

[10] T. L. Kelley and Eugene Shen, General statistical principles, *The Foundations of Experimental Psychology*, 1929, 849.

Finally the factor of suggestion must not be overlooked as a source of the high correlations. 'Bright' suggests joyous, happy, pleasant states, while 'dull' suggests dead, unpleasant states. So a color judged as pleasant would be more apt to be suggestive of 'bright' pressures than one judged as unpleasant. It is not assumed that 'memory' played any part from the reports under the one set of instructions to those of the other. The 'suggestion' might well be immediate and attach to the color directly. In the same manner 'personal' might suggest pleasantness and 'impersonal' unpleasantness.[11] If a stimulus is pleasant it may be (if an *O* must choose between 'bright' and 'dull' or between 'personal' and 'impersonal') 'bright' rather than 'dull,' 'personal' rather than 'impersonal.' Thus high correlations might well be established between hundreds of pairs of words having no more connection psychologically than the affects and pressures and 'personal-impersonal.'

[11]It should be understood we did not obtain our high correlation between affects and 'personal-impersonal' by training our *O*s. In fact, they did not understand 'what it was all about' as they afterwards reported. The suggestion we are speaking of we believe to be immediate and has its locus in the instructions, setting of the experiment, and the *O*, particularly in the *O*.

ATTENTION AND BODILY SWAY

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In 1925 Fearing reported the results of a systematic investigation of the relation between attention and bodily sway,[1] which indicated that the subjects were better able to maintain a given posture of the body when attention was directed away from the task of maintaining it than when attention was directed to that task. These results were in part negated and in part confirmed by Skaggs and Eichkern,[2] in 1928, who found that some subjects swayed less when their attention was directed to the immediate task of muscular coördination, and that others swayed less when their attention was directed to extraneous matters. It was in the hope of explaining these equivocal results that the present study was undertaken.

Method and Procedure

Apparatus. Since the apparatus has already been described,[3] brief mention of it here will suffice. There are three integral parts to it: a body-piece, a meter stick with a cardboard platform attached to the upper end, which is fastened to *S*'s back—the platform projects about 2 in. above his head; a recording sheet,[4] smoked or sensitized paper, which is fastened to the top of the platform; and a stylus, an adjustable overhead rod that moves freely in the vertical direction in a sleeve that prohibits horizontal play, which is brought to bear upon and removed from the sensitized paper as desired.

General procedure. *S* stood under the overhead piece with his feet in a constant and standard position, *i.e.* with his heels 2 in. apart and turning outward at an angle of 20°. He was instructed to assume a comfortable erect position and to signal by saying "Now" when he had found one that he thought he could maintain. At the signal, *E* brought the stylus to bear upon the sensitized paper and exactly 1 min. later removed it. After an interval of from 1–2 min. the procedure was repeated. All the experiments were made, unless otherwise stated, with eyes open.

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[1] F. S. Fearing, Factors influencing static equilibrium, *J. Comp. Psychol.*, 5, 1925, 1-25.

[2] E. B. Skaggs and G. C. Eichkern, Some studies of body sway, *Mich. Acad. Sci., Arts, and Letters*, 10, 1928, 369-79.

[3] Skaggs and Eichkern, *op. cit.*

[4] In the first part of the work we used smoked paper, in the later part we used a sensitized gelatin-film paper.

Movements of the body were thus graphically recorded. The greatest deviation from the initial position was taken as the measurement of bodily sway. We could not, although that might be highly desirable, measure the total amount of bodily movement with our recording apparatus. All the measurements were made in millimeters.

Experiment I

Records were obtained in Experiment I under three different instructions: (1) with attention directed to the task of maintaining position (MP); (2) with attention directed to the task of mental multiplication (MM); and (3) with attention directed to the task of reading (AR).

Subjects. Twenty-one *Ss*, 10 women and 11 men, all college students, served in this part of the study. None of the *Ss* had had any previous training in the tests.

Procedure. Every *S* came to the laboratory 5 times. Six records were obtained at every meeting, *i.e.* 2 under every one of the 3 instructions. In all, therefore, 30 records (10 under every instruction) were obtained for every *S*. The order of the tests was rotated at each meeting so as to equalize the effects of practice and other errors due to the temporal position of the tests in the day's series.

Results: (*a*) *Objective.* The data of Table I show that both groups, men and women, are better able to maintain a standing position with attention directed to mental multiplication. The group averages indicate this clearly. If, instead of comparing group averages, we consider the individual cases (not given in the table) we find that 10 of the 11 men, and 7 of the 10 women, sway less when attention is directed away from than when it is directed to the task of maintaining position.

A comparison of results presented in Table I shows, on the basis of group averages, that the *Ss* swayed more under the AR instruction than under the MP. Inspection of individual averages (not given in the table) shows that 7 of the 11 men, and 6 of the 10 women, sway more under the AR instruction.

Under the conditions used in this section of our study, then, we find that *Ss* sway less with attention directed to mental multiplication but more with attention directed to reading, as compared with attention directed to the task of maintaining position.

(*b*) *Introspective.* A study of the introspective reports of the *Ss* indicated that there was great need to define more specifically the condition of directing attention to maintaining position. The instructions to "keep attention directed to the task of maintaining position"

were interpreted in various ways by different *Ss*. One kept saying to himself, "Hold fast, steady now. I must keep this position," and so on. Another visualized the overhead piece, translating his kinaesthetic sensations into visual imagery. Another attended to

TABLE I

GROUP AVERAGES UNDER THE DIFFERENT INSTRUCTIONS

		Attention on		
	No.	Maintaining Position (MP)	Mental Multiplication (MM)	Reading (AR)
Men	11	23.9±5.9	18.4±4.1	24.5±6.2
Women	10	30.4±5.3	25.9±4.3	32.2±8.0

the muscle strains from his leg muscles. Still another watched a spot on the wall. It was obvious that while all of the *Ss* directed their attention away from the task when so instructed there was no uniformity regarding the content of attention when they were instructed to attend to the task of maintaining position.

EXPERIMENT II

The results of Experiment I demonstrated the necessity of strictly defining the instruction, "attend to maintaining position." With that in mind a second series of experiments was performed.

Procedure. The *Ss* were requested to attend, under the instruction to maintain bodily position, to the strain sensations in the leg muscles and to make their readjustments by carefully noting and reacting to the shifting tensions. This instruction, which is designated as the MP_s instruction, was alternated with the instruction to attend to the task of mental multiplication (MM).

Subjects. Twenty-two college students, 11 men and 11 women, served as *Ss*. Eight records were obtained from every one of them—4 from each instruction at 2 experimental periods. One *S* was started with one instruction, the next with the other instruction, and so on, alternating throughout the group. The tests under both instructions were performed with closed eyes.

Results. Table II, which gives the averages for the group, indicates that there is less sway with attention directed to mental multi-

TABLE II

SHOWING GROUP AVERAGES UNDER THE DIFFERENT INSTRUCTIONS

(Eyes Closed)

	Attention on	
No. *Ss*	Maintaining Position and Strain (MP_s)	Mental Multiplication (MM)
22	30.6±6.2	27.8±6.9

plication. A study of individual cases, which are not presented, shows that in 14 out of 22 cases the MM instruction results in the better record. Although all of our *S*s directed their attention to the same mental content, the results found in this experiment are substantially the same as those found in the preceding experiment.

Experiment III

In view of the results of Experiment II it seemed desirable to make a more intensive study of a few individuals who were fairly well trained psychologically and who could give us more trustworthy introspections. While most *S*s swayed less with distracted attention some swayed less with attention directed to the task of maintaining position. The question arose as to whether these differences were due to the fact that we had secured only a small number of records or whether they were indications of important individual differences.

Subjects. Six *S*s (*A*, *J*, *K*, *M*, *N*, and *P*) were used in this study. *A* had had a great amount of training in this work. *M* had had considerable previous training. The other four had had 20 previous tests of bodily sway. After a few preliminary trials 60 records were obtained from every *S*, *i.e.* 20 records under every one of the 3 instructions. Six records, 2 for every instruction, were secured at every meeting. The order of the tests was rotated at each meeting in order to equalize the effects of practice and fatigue.

Procedure. Bodily sway was measured under 3 instructions: (1) under the MP instruction, as that was defined in the previous section; (2) under the MM_s instruction, *i.e.* under the distraction of easy tasks in mental multiplication (the multiplication of 2-place by 1-place digits); and (3) under the MM_h instruction, *i.e.* under the distraction of difficult tasks in mental multiplication (the multiplication of 2-place by 2-place numbers). In all the distraction experiments *E* called out the numbers and *S* returned the answers orally as soon as solved, when another problem was immediately given him.

Results. Table III presents the means and mean variations for every one of the 6 *S*s. While the averages for the whole group are given we are chiefly interested in the individual averages. *A* and *M* do persistently better under the MP instruction than under the distracted conditions. On the other hand *J*, *K*, and *N* do persistently better in the distracted conditions. *P*, alone, gives uncertain results. He swayed less under the MP instruction than under the MM_e, and more under the MP than under the MM_h.

As a further check upon the reliability of the individual differences we fractionated our data, as shown in Table IV. We have presented the mean for the first and the last 10 records under every

one of the 3 instructions. If we are dealing with real and deep-seated individual differences here, the practice effect should not change matters in any material way.

TABLE III

SHOWING AVERAGES FOR 6 TRAINED *Ss*

S	Attention on: Maintaining Position (MP)	Easy Problems (MM_e)	Hard Problems (MM_h)
A	20.8± 5.9	25.7±6.3	22.0± 5.5
J	41.2±12.0	31.4±7.3	35.3± 7.3
K	23.4± 5.0	19.1±4.6	18.1± 5.5
M	26.4±10.3	28.8±6.6	28.3±10.1
N	30.4±12.3	19.5±6.9	16.7± 4.8
P	19.2± 6.1	24.0±8.5	16.8± 5.1
Mean	26.9± 5.93	24.8±3.88	22.9± 5.97

TABLE IV

SHOWING AVERAGES FOR FIRST AND LAST TEN RECORDS

S	First ten: MP	MM_e	MM_h	Last ten: MP	MM_e	MM_h
A	20.3	23.1	19.5	21.3	28.3	24.5
J	49.6	31.8	33.1	32.7	31.0	37.5
M	24.4	27.4	27.5	28.3	30.1	29.0
K	24.3	22.3	18.6	22.5	15.9	17.6
N	38.3	19.3	16.6	22.5	18.8	16.8
P	20.9	19.6	15.1	17.5	28.4	18.4

Let us first compare averages for the first and last 10 trials for every *S* under the MP and the MM_e instructions. With the single exception of *P* all the *Ss* run true to form in each half of the series. *P* sways less under the MM_e instruction in the first 10 trials but the situation is just reversed in the second 10.

If we compare the averages for the first and last 10 trials for every *S* under the MP and the MM_h instructions we find much the same thing. There are, however, two exceptions. *J* sways less under the MM_h instruction in the first 10 trials, and more in the last 10. *P* sways less under the MM_h instruction in the first 10 trials, and more in the last 10.

All in all, we believe that our data suggest that we are dealing with rather deep-seated individual differences, differences which persist in spite of considerable practice. Most *Ss* do better with attention

distracted but some do better with attention directed to the task of maintaining bodily position. So much is clear but there yet remains the difficult task of accounting for the fact that some *S*s sway less when their attention is distracted and that others sway less when they attend to the task of maintaining bodily position.

It occurred to us that the explanation of our results might be found in the fact that some *S*s tense their muscles more than others. We proceeded to study this factor in Experiments IV and V.

Experiment IV

The *S*s in Experiment IV were given the general instruction to attend to maintaining bodily position, and the specific instructions to stand in one series of experiments with arm and leg muscles normally tensed, and in the second series with muscles tensed a little more than normal.

After a preliminary practice series of experiments, during which the *S*s adjusted themselves to the instructions, the main experiments were conducted. The instructions were used alternately; some *S*s beginning with one, and some with the other.

Two groups of *S*s, college students, were used; 15 served with eyes open, and 14 with eyes closed.

Results. The results of Experiment IV are given in Table V. The group averages show that the sway of the body was greater for both groups of *S*s when the muscles were tensed. The individual records (not given in the table) reveal that this relation held for 14 of the 15 *S*s who served with open eyes, and for 8 of the 14 *S*s who served with closed eyes.

Experiment V

A final experiment was made with 4 trained *S*s. Twenty records were secured for every *S* under each of the instructions of Experiment IV. The results are summarized in Table VI. Three of these *S*s did much better in the normally tense condition whereas one, *M*, did better in the more tense condition.

These data indicate, we believe, that the relative tension of the muscles is an important factor in accounting for the differences which we have found between individuals in our study. The tentative suggestion is offered that most *S*s sway more when attention is directed to the task of maintaining position because, in part at least, they tense their muscles more under that condition than when their

attention is distracted. Those *Ss* who sway more with distracted attention may tense their muscles more under this condition. The question of the relation between direction of attention and degree of muscle tension needs systematic investigation in which a more exact and objective method of measuring muscle tension is used.

TABLE V

GROUP AVERAGES WITH EYES OPEN AND CLOSED

Eyes	No. *Ss*	Muscles	
		Normal	Tensed
Open	15	27.3±7.3	37.3±5.5
Closed	14	35.0±7.6	37.4±3.7

TABLE VI

AVERAGES FOR HIGHLY TRAINED *Ss*

Muscles	Subjects			
	A	*J*	*L*	*M*
Normal	20.1±5.1	26.2±8.2	25.7±9.0	22.4±6.1
Tensed	26.4±5.7	29.8±5.8	28.0±8.3	19.0±5.3

SUMMARY

Measuring bodily sway in terms of the greatest amount of deviation from an initial position during a unit of time (1 min.) the following conclusions are indicated by our data:

(1) The majority of unpracticed *Ss* sway less when their attention is entirely distracted from the processes of coördinating the body. A few sway more when attention is distracted.

(2) Our practiced *Ss* are evenly divided; one group does persistently better with attention distracted from the task of coördinating the body whereas the other group does better when attention is directed to the task of maintaining body position.

(3) Practically all *Ss*, unpracticed and practiced alike, do better when standing with "just normal muscular tension" than when standing with "greater than normal tension."

(4) The suggestion is offered that varying degrees of muscular tension, associated with the direction of attention, may be an important factor in accounting for the individual differences which we have found.

THE DEPENDENCE OF EYE-HAND COÖRDINATIONS UPON EYE-DOMINANCE

By F. H. Lund, Temple University

Most of the literature dealing with preferential usage, or with dominance of one side of the body over the other, has been concerned with its most conspicuous feature; namely, handedness. It is to this form of dominance, again, that chief consideration has been given in the theories advanced to account for the genesis of body asymmetries.[1] In addition to handedness only two other forms of functional dominance—eyedness and veering—have been given serious consideration. The data and findings, as related to these three forms of dominance, *i.e.* handedness, eyedness, and veering, are summarized briefly.

Handedness. Dextrality, or right-handedness, according to the earliest writers on the subject, characterizes 94% to 98% of the population, and sinistrality, or left-handedness, 2% to 6% of the population. More recent investigators have regarded these estimates as too high for right-handedness. It is maintained that the very low estimate of about 5% left-handedness is based solely on the more obvious dextral and sinistral usage, and that many of the congenitally sinistral (particularly the mildly sinistral) have been changed to dextral because this is the more frequent, and because dextrality has the advantage in a world which is largely dextral. Jordan, who was one of the first to depart from the earlier estimates, and the first to suggest that handedness follows Mendelian principles, maintained (in 1914) that right-handedness is now present in a ratio of 4:1.[2] Ramalay,[3] subsequent to Jordan's statement, placed the ratio at 6:1. Von Bardeleben,[4] in 1914, gave the number of children who have been changed from left-handed to right-handed as 26%. Parson,[5] working from his data on eyedness, reaches the same conclusion, maintaining that the natively right-handed and the natively left-handed are probably present in the ratio of 3:1, or tend to approximate this in accordance with Mendelian principles. As far as actual usage is concerned, a conservative estimate would probably place the distinctly right-handed at 90%, the ambiguous and ambidextrous at 5%, and the distinctly left-handed at 5%.

*Accepted for publication June 15, 1931.

[1]One of the best summaries of these theories is to be found in B. S. Parson, *Lefthandedness*, 1924, 2 ff.

[2]H. E. Jordan, Hereditary lefthandedness, *J. Genet.*, 4, 1914, 67.

[3]F. Ramalay, Inheritance of lefthandedness, *Amer. Nat.*, 47, 1914, 730.

[4]K. von Bardeleben, Lefthandedness, *J. of Hered.*, 5, 1914, 312.

[5]Parson, *op. cit.*, 99 ff.

Eyedness. Ocular dominance, or the tendency for one eye to become the directing and controlling eye wherever close coördination of eye and hand is required, seems first to have been recognized by Humphrey, who referred, in 1861, to "the correspondence of function between eye and hand."[6] In 1881, LeConte stated that "people are right-eyed as well as right-handed, and left-eyed as well as left-handed."[7] Gould, who has written extensively on this subject, contends that "handedness depends on which is the better seeing eye."[8] Parson examined 877 school children for eyedness and found that while eyedness approximates the Mendelian ratio, handedness, so far as actual usage is concerned, does not. He concludes that "to the extent that virtually all the righteyed are at the same time righthanded, we are justified in believing that the natively lefteyed, irrespective of their present manual preference, were all originally lefthanded."[9] Repeating Parson's tests, Cuff has more recently arrived at similar conclusions. A report of his study states that "the manoptometer indicated that 20% to 30% of the children were probably left-handed natively, and all were left-eyed except one case—also that 100% of the right-eyed were found to be right-handed."[10]

Veering. A third form of functional dominance recently investigated is the tendency to veer, or to turn either right or left according to lateral bias, when normal orienting cues are absent. The consistency of right-veering or left-veering in particular individuals indicates a distinct form of dominance. According to Asa Schaeffer this phenomenon is not peculiar to humans. In his conclusions, following wide observations upon lower forms, he states summarily that "no forward moving organism has been found which does not move in a spiral path when there are no orienting senses to guide it."[11] The veering or spiralling tendency of humans was tested in an earlier experiment by Lund.[12] The results of this study indicated no close relationship between veering tendency and handedness, or between veering and eyedness. On the other hand, consistent veering in a given direction from intended line of movement on the part of blindfolded *Ss*, showed a high correspondence with structural dominance on the side opposite to the line of deviation. Thus, those who were right-veering showed more frequent structural dominance of the left side, and *vice versa*. Right-veering, indicating left-dominance, was more frequent than left-veering. This greater frequency of left-dominance is paralleled by more frequent structural dominance (greater length) of the left leg. Most of the subjects with longer left leg were found to have a longer right arm. This fact of 'crossed symmetry'

[6]G. M. Humphrey, *The Human Hand and the Human Foot*, 1861, 8.
[7]J. LeConte, *Sight*, 1881, 107.
[8]G. M. Gould, *Right-Handedness and Left-Handedness*, 1908, 98.
[9]Parson, *op. cit.*, 114.
[10]N. B. Cuff, from a report on the 25th annual meeting of the Southern Society for Philosophy and Psychology, this JOURNAL, 42, 1930, 459-460.
[11]Asa Schaeffer, Spiral movements in man, *J. Morph. and Physiol.*, 45, 1928, 297 ff.
[12]F. H. Lund, Physical asymmetries and disorientation, this JOURNAL, 42, 1930, 51-62.

(longer arm on one side coupled with longer leg on the other) has been reported by Hasse and Dehner,[13] and by Guldberg.[14]

That structural and functional dominance do not necessarily go together may be seen from the fact that, despite greater frequency of structural dominance of the left leg, most people are, nevertheless, right-footed, and from the fact that functional dominance of the right eye does not always mean that this eye is structurally superior to the left.

Problem

The purpose of the present investigation was to secure further data upon the nature and interrelation of dominant traits. The two forms of dominance especially under consideration were those of handedness and eyedness. To determine the interdependence of these under actual test conditions seemed desirable, particularly in view of the cause and effect relationship which is said to exist between them. Before proceeding to a more precise statement of the problem it will be necessary to consider briefly some of the more pertinent physiological data relating to eye and hand adjustments.

Man's vision is, in the main, binocular. It is binocular particularly in adjustments to distance, to shape, and to form. It is monocular in adjustments to objects seen in indirect vision. In most of the lower biological forms, on the other hand, vision is distinctly monocular, since the eyes are so far apart and the visual axes so divergent that there can be no common field of view for the two eyes. In such cases the eye on the right side controls the movements of the right side of the body, and the eye on the left controls the movements on the left side. "This harmony of function," says Parson, "survives in man at least to the extent that righteyedness accompanies righthandedness, and lefteyedness accompanies lefthandedness. . . . With these facts in mind we are prepared to learn from pathology that the all-important motors centers for the hand and arm and for some of the more expert visual operations on the preferred side lie close together in the left hemisphere for the righthanded, and in the right hemisphere for the lefthanded."[15] This grouping of faculties in one hemisphere would seem to be true for handedness, eyedness, and for written and spoken speech.

From the above we see that eye-dominance is closely linked with monocular space perception and with the general problem of sided-

[13]C. Hasse and M. Dehner, Unsere Truppen in körperliche Beziehung, *Arch. f. Anat. u. Entwicklungsgesch.*, 16, 1893, 249-256.

[14]G. A. Guldberg, Über die morphologische und functionelle Asymmetrie der Gliedmassen beim Menschen und bei den hoheren Vertebraten, *Biol. Centralbl.*, 16, 1896, 806-813.

[15]Parson, *op. cit.*, 22.

ness. Space perception, we have noticed, is monocular in the case of all objects seen indirectly, for the reason that such objects will appear double when viewed with both eyes. Accordingly, adjustments to these objects are guided by one eye only, the dominant eye. Particularly is this true in the case of eye-hand adjustments calling for close coördination. In this event it is the *hand* that is seen indirectly, since the eyes are trained upon the point of adjustment rather than upon the adjusting member. That the directing and controlling eye should be on the same side as the controlling member would seem to be in the interest of efficiency.

But now might it not be well to determine just how far these general facts of monocularity enter into specific adjustments, and, also, how far efficiency will be impaired if the so-called' natural grouping of faculties' is broken up by placing the individual in the position where he must use the non-dominant eye to guide the dominant hand? It was to test this general proposition that the experiment herein described was undertaken.

Method and Procedure

Since the familiar aiming test or target test conforms with all the requirements of precise eye-hand coördinations, it was introduced as a measure of the phenomenon under consideration. The target blank (see Fig. 1) was fastened to the wall at a height corresponding to the *S*'s shoulder. *S* was placed with his shoulder squarely in front of the target, and at such a distance that his pencil would just strike the target when his arm was fully extended. He was told to strike in time with the beat of a metronome (set to beat 90 times per min.), following a two-beat rhythm, so that the pencil would hit the target at one beat and would be drawn back at the next. He was advised to make full, smooth strokes, bringing the hand clear back to the shoulder each time. The crosses were struck at successively and in the order indicated by the numbers in Fig. 1.

Every *S* was given 9 tests: 3 with both eyes open; 3 with the right covered by means of a small conically shaped cap; and 3 with the left eye similarly covered. Following the target tests every *S* was tested for eyedness, using as a measure the monoptometer, a full description of which is given in a previous article.[16]

Of the 247 *S*s used in the experiment, 128 were juniors or seniors in high school, and 199 were college students. In analyzing the data the *S*s were grouped according to handedness and eyedness as 'right-eyed dextrals,' 'left-eyed sinistrals,' 'right-eyed sinistrals,' 'left-eyed dextrals,' and 'binocular and ambiguous.'

[16]Lund, The monoptometer: A new device for measuring eye-dominance, this Journal, 44, 1932, 181 ff.

Results

The scores for the various groups are shown in Table I. They represent the average errors made on the target test (1) when using both eyes. (2) when using the dominant eye, the non-dominant being covered, and (3) when using the non-dominant eye, the dominant being covered. These scores were arrived at in the following manner. The total amount of mm. deviations from the point aimed at in ten successive thrusts was first determined for each *S*. Then

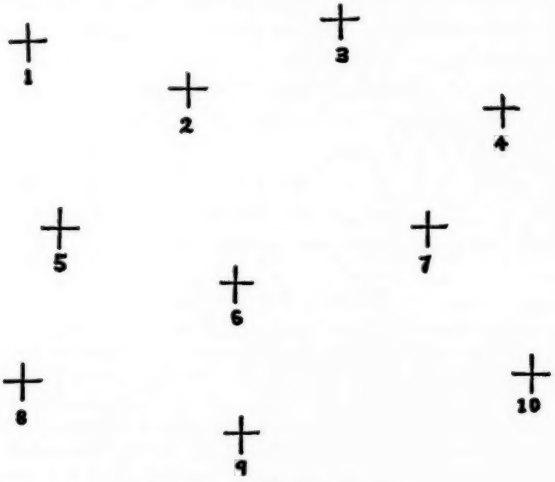

Fig. 1. The Target Blank
(Actual Size)

the average record for every *S* in the three types of test was determined. From these averages a grand average for all *S*s belonging to a particular group was calculated for each type of test. Thus, it will be clear that in the case of the scores entered in Table I a high number really indicates a poor record, since this number represents the total amount of deviation (measured in mm.) in 10 successive thrusts.

The results are interesting in several respects. First, in the fact that the best scores are made with both eyes open—this, despite the contention that we are in effect one-eyed in such adjustments and that the non-dominant eye really acts as an interference. The

second point of interest is the decided and consistent advantage which the dominant eye—irrespective as to whether it is the right or the left—has over the non-dominant eye in the case of the unmixed groups, that is, the right-eyed dextrals and the left-eyed sinistrals. Third, this advantage of the dominant eye over the non-dominant is not apparent in the case of the left-eyed dextrals, which is a mixed group.

TABLE I

SHOWING THE RESULTS OF THE TARGET OR AIMING TEST AND THE RELATION WHICH THESE BEAR TO EYEDNESS AND HANDEDNESS

Classification	Percent	Eye used					
		Both		Dominant		Non-dominant	
		Score	P.E.	Score	P.E.	Score	P.E.
Right-eyed dextrals	69.1	55.1	1.9	61.2	2.2	67.1	2.4
Left-eyed sinistrals	4.2	55.4	2.0	59.6	1.9	62.2	2.0
Right-eyed sinistrals	.8	56.2		60.9		63.1	
Left-eyed dextrals	21.1	55.0	1.8	61.7	2.7	61.9	3.1
Binocular and ambiguous	4.7	59.7	2.6	69.5 (right)	3.2	67.1 (left)	3.3

In general, then, there can be no doubt about the fact of dominance. It is just as apparent from these tests of efficiency in actual eye-hand adjustments, as it is in the case of the monoptometer tests. But it must be equally apparent (1) that we are binocular as well as monocular even in so simple an adjustment as the one under consideration, and (2) that highest efficiency is achieved when the two forms of vision are permitted to coöperate, as they apparently do in such situations. It would probably be futile to try to single out the aspects of the perceptual process which are binocular and the ones which are monocular, but it is likely that in a particular situation such as the one represented in the aiming test, binocular vision functions chiefly in judging distance, and monocular vision in guiding the movements of the hand or objects seen in indirect vision. In any event, it would be safe to conclude that there are probably very few if any adjustments of this type which do not involve both types of vision. It would appear equally certain that the use of both eyes under these conditions has the advantage. There was not a single subject for whom this was not true.

The advantage of the dominant eye over the non-dominant eye, while quite definite and decided when averages are considered, did

not always hold for individual cases. About one out of every four showed a margin of superiority in favor of the non-dominant eye. With a larger number of tests for such subjects this advantage might be reversed. In the case of the left-eyed dextrals the non-dominant eye yielded the better score in almost as many cases as the dominant eye. This would seem to indicate that the right eye, though not the dominant eye in the case of these subjects, has nevertheless been brought more into use than might otherwise have been the case, since it is the nearer eye so far as the functionally dominant hand is concerned. The data in this case point definitely to the advantage of consistent lateralism.

Summary

The results of the target test, while supporting the general fact of ocular dominance as indicated by monoptoscopic tests, do not support the theory that vision is distinctly monocular in all adjustments calling for high precision of eye and hand, or requiring that the coördinating member be guided through indirect vision. That the dominant eye plays a more important rôle than the non-dominant is evident from the fact that the scores made with the dominant eye (the non-dominant being covered) were better than those for the non-dominant eye. This was true irrespective of which eye was dominant. That binocular as well as monocular vision enters into eye-hand adjustments of this type is apparent from the fact that the best scores were made when both eyes were in use. Thus, ocular dominance is not the simple phenomenon it would at first seem to be. It is likely that we are not distinctly binocular or monocular with respect to any function. That the left-eyed dextrals should have made about equally good scores with the non-dominant as with the dominant eye is interesting in that it indicates that the shift from congenital left-handedness to trained right-handedness results in greater training of the right eye—on account of its advantage of position—than might otherwise be the case.

PRIMACY AND RECENCY IN THE LEARNING OF VISUAL DIAGRAMS

By LELAND W. CRAFTS, New York University

The object of the present experiment was to discover whether primacy and recency—terms referring solely to the temporal position of members of a series—would have significance in the learning of certain visual diagrams. The experimental literature relevant to primacy and recency is large and covers a wide range and variety of material. The present investigation differs from previous experiments in several respects. It differs from the great majority of the earlier studies in employing perceptual material instead of verbal or motor. It differs from the experiments dealing with perceptual materials in that the series to be learned consisted of the successive parts of a single diagram rather than sequences in which every member was a separate and distinct unit. It differs from all previous studies, in so far as the writer is aware, in having the *S*s graphically reproduce the materials presented. In brief, the present experiment differs from those that have gone before in both the nature and the treatment of the material used.

It is, we believe, of importance to see whether current generalizations as to the factors of influence in learning may be extended to new situations and new types of material. If they can be, then they gain in certainty and range of application; if they cannot be, then some sort of amendment or qualification is of course suggested.

PROCEDURE

Subjects. The *S*s were members of 8 classes in elementary psychology. The total number used was 324.

Materials. The materials for learning were visual diagrams identical with those used in a previous experiment and described in detail in an earlier number of this JOURNAL.[1] It may, therefore, suffice here to say that they were: (1) Sixteen circles arranged as irregularly as possible and occupying 16 of 64 small squares into which a large square had been divided. (2) Sixteen separate, unrelated lines, diagonal, horizontal and vertical, also arranged as irregularly as

*Accepted for publication July 17, 1931.

[1] L. W. Crafts, Whole and part methods with visual spatial material, this JOURNAL, 44, 1932, 527-528.

possible and connecting 32 of 49 points, the latter all equidistant from each other. (3) A single, complete figure, composed of 16 lines also connecting 32 of 49 equidistant points and made as irregular and asymmetrical as possible. An equal number, namely 4, of circles, unrelated lines, or lines that were elements of a figure was allotted to every quarter of every diagram. By 'quarter' is meant the quarters that would be formed if a diagram were bisected horizontally and vertically by two lines at right angles to each other. These quarters will hereafter be called 'parts' and will be separately identified as follows: The upper left quarter will be called Part A, the upper right Part B, the lower right Part C, and the lower left Part D. An effort was made to have all four quarters about equal in difficulty.

Presentation. The material was displayed in the manner described in the earlier experiment referred to above. Rotation of a large sheet of cardboard, from which an area had been cut of a size such as would show only a quarter of the diagram at a time, made possible the exposure of every part separately for the predetermined period.

The exposure times for the various materials were as follows: circles, 23 sec. per quarter; lines, 30 sec.; and figures, 10 sec. These times permitted only partial learning for a majority of the *S*s. The presentation was continuous, *i.e.* there was no interval whatsoever between the exposure of any two successive parts.

Rotation of parts. In order to eliminate the varying difficulty of the parts as a disturbing factor, every diagram was shown to at least four different groups of *S*s. To one of these, hereafter called Group 1, Part A was presented first, then Parts B, C, and D in that order. For Group 2 the order was Parts B, C, D, and A. For Group 3, it was Parts C, D, A, and B; and for Group 4 it was D, A, B, and C. Hence every one of the four parts occupied once every one of the four possible temporal positions in the series of parts. The presentation, it should be noted, was always in a clockwise direction.

Reproduction and scoring. The *S*s received blank copies of the diagrams and instruction sheets briefly describing the nature of the material and the procedure to be followed. They were also informed as to the order in which the parts would be shown. When all four parts had been presented, the *S*s immediately reproduced what they could of the diagram. They were allowed about 3 min. to make the reproduction. Every correct detail was scored 1. No partial credits were given.

Results

The results are given in Table I. The part presented first to a group is called Part 1, regardless of whether it was Part A, B, C, or D; similarly the parts presented second, third, and fourth or last are called Parts 2, 3, and 4 respectively. The letters in parentheses beside the averages show which quarter every part was. N was 'corrected' by eliminating all *S*s who made the perfect score of 16 on the whole diagram, for obviously such scores do not permit any differential learning of the various parts to show itself. The averages given are for the corrected N only.

Inspection of Table I shows that every group made higher scores on Parts 1 and 4 than on Parts 2 and 3 except as follows: with Lines, Group 2; with Figure 1, Groups 1, 3 and 4; with Figure 2, Group 4. The numerous exceptions obtained with Figure 1 are due obviously

TABLE I

GIVING THE AVERAGE SCORES BY PARTS, AND FOR THE WHOLE DIAGRAM, FOR ALL GROUPS AND ALL MATERIALS

	Group	Original N	Corrected N	Part 1	Part 2	Part 3	Part 4	Whole Diagram
Circles	1	37	29	3.3(A)	3.0(B)	2.6(C)	3.1(D)	12.0
	2	48	33	3.6(B)	3.4(C)	2.5(D)	3.5(A)	13.0
	3	38	34	3.3(C)	2.4(D)	3.0(A)	3.5(B)	12.2
	4	47	28	3.0(D)	2.8(A)	2.9(B)	3.8(A)	12.5
	All	170	124	3.30	2.90	2.75	3.48	12.43
Lines	1	36	35	2.2(A)	1.5(B)	1.5(C)	2.5(D)	7.7
	2	45	39	2.3(B)	2.7(C)	2.8(D)	2.7(A)	10.5
	3	36	35	2.7(C)	2.2(D)	2.4(A)	2.8(B)	10.1
	4	46	44	3.1(D)	2.1(A)	2.2(B)	3.2(C)	10.6
	All	163	153	2.58	2.13	2.23	2.80	9.73
Figure 1	1	34	30	3.4(A)	2.1(B)	1.5(C)	1.4(D)	8.4
	2	41	33	3.0(B)	1.9(C)	1.8(D)	3.7(A)	10.4
	3	35	31	2.1(C)	1.5(D)	3.2(A)	2.2(B)	9.0
	4	46	37	2.3(D)	2.5(A)	2.2(B)	1.8(C)	8.8
	All	156	131	2.70	2.00	2.18	2.28	9.15
Figure 2	1	43	43	1.7(A)	1.1(B)	0.9(C)	1.3(D)	5.0
	2	44	44	1.5(B)	1.3(C)	1.3(D)	1.4(A)	5.5
	3	30	30	1.5(C)	1.3(D)	0.9(A)	1.4(B)	5.1
	4	37	37	1.4(D)	1.3(A)	1.2(B)	1.2(C)	5.1
	All	154	154	1.53	1.25	1.08	1.33	5.18

to the fact that Part A was much the easiest of the four parts; every group secured its highest score on that part regardless of the temporal position in which it was shown. For this reason Figure 2 with parts more nearly equal in difficulty was devised and shown to four new groups entirely without experience in learning these diagrams. With this figure all groups except Group 4 made their highest score on Parts 1 and 4.

The average scores for all groups combined show that without exception with all materials the scores made on Parts 1 and 4 were

higher than those made on Parts 2 and 3. With the Circles and the Lines, the score on Part 4 was slightly higher than that on Part 1; with both figures, however, Part 1 obtained the higher score.

Since the differences between the scores on the various parts were necessarily slight, it seemed to the writer preferable not to compute their S.D. but rather to depend upon the consistency and uniformity of the results for indication of their validity.

Discussion

The results show that primacy and recency were significant in the learning of the material employed in the sense that the parts presented first and last were better learned than those presented in the two intermediate temporal positions. For recency to have such value is a finding too familiar to require further comment here. As to the value here indicated for primacy, our results are in harmony with those of Robinson and Brown,[2] Jenkins and Dallenbach,[3] Sauer,[4] and Willoughby,[5] and in opposition to those of Thorndike,[6] and Burnett and Welch.[7] In connection with the question as to its significance, Burnett and Welch, summarizing the results of their experiment with unpronounceable nonsense syllables, write as follows: "There was good reason to regard Primacy as a factor in association until experimental control was obtained over the *O*'s tendency to 'think backward' during the exposure of a series to be remembered. When this tendency was prevented, by the coöperation of the *O*, the alleged effect of primacy disappeared."[8] In the present experiment, however, it seems probable that the nature of the material, the continuity of the presentation and the immediacy of the reproduction must have prevented much if not all of any such thinking backward. Hence the value of primacy here indicated is probably

[2]E. S. Robinson and M. A. Brown, Effect of serial position upon memorization, this Journal, 37, 1926, 538-552.

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[4]F. M. Sauer, The relative variability of nonsense syllables and words, *J. Exper. Psychol.*, 13, 1930, 235-246.

[5]R. R. Willoughby, Incidental Learning, *J. Educ. Psychol.*, 20, 1929, 671-682.

[6]E. L. Thorndike, The influence of primacy, *J. Exper. Psychol.*, 10, 1927, 18-29.

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The purpose of the experiment was to discover whether primacy and recency would have significance in the learning of certain visual diagrams. Irregular arrangements of circles and of lines and irregular figures were divided into four parts. These parts were presented once, in a continuous series; immediate graphic reproduction followed. The factor of the varying difficulty of the parts was controlled by employing a number of groups sufficient to permit every part to be shown in every one of the four possible temporal positions. The results show that with no important exceptions the groups secured higher average scores on the parts presented first and last than they did on those occupying the two intermediate positions. Hence the significance not only of recency but of primacy as well for the learning of this visual material under the experimental conditions here employed seems to have been demonstrated, and the scope of the current generalizations concerning their value may to this degree be extended.

[9] Jenkins and Dallenbach, *loc. cit.*

[10] M. Foucault, Les inhibitions internes de fixation, *Année psychol.*, 29, 1928, 92-112.

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THE SIGNIFICANCE OF EARLY PERSONAL HISTORY FOR CERTAIN PERSONALITY TRAITS

By CHARLES K. A. WANG, University of Chicago

The purpose of this investigation is to make an exploratory study of the significance of selected items of fact in one's early personal history for certain of his personality traits.[1] It is an attempt to answer the question, "What facts in a person's early life are significant in the formation of some of his personality traits?" In doing so, we are interested not so much in individual cases as in the group differences of the various personality classifications.

For information on early personal history a special questionary was prepared. No attempt was made to have it exhaustive; care was taken, however, to include principally items which could be answered with a minimum of subjective judgment.

For information regarding the personality traits, the following tests were used: (1) Allport's Ascendance-Submission Test;[2] (2) Freyd's Test of Introversion-Extraversion,[3] (3) Moss's Test of Social Intelligence,[4] (4) Pressey's X-O Test of Emotionality,[5] and (5) Otis's Test of Suggestibility.[6]

Nearly 600 undergraduate students in the University of Chicago provided data. After eliminating those whose records were incomplete and those over 25 yr. of age, a total of 358 cases, 203 men and 155 women, remained which were used as the basis of this study.

The Otis test of suggestibility was given to the *Ss* during one of their class periods. The other tests, together with the personal history questionary, were permitted to be filled out at home without supervision, and returned later.

Personality traits. For the sake of economy of space, the distributions of raw scores on the 5 personality tests are not here presented. It was found, however, that in most cases our central tendencies differed considerably from those given by the respective authors for comparable groups, *i.e.* for college students. Table I gives a comparison of the median scores for our *Ss* and those given by the respective authors.

*Accepted for publication July 1, 1931.

[1]This paper is abbreviated from a doctoral dissertation which is on file, under similar title, in the University of Chicago Library. For a copy of the questionary used in this study and for a detailed presentation of the data, the reader is referred to the thesis which may be borrowed under the usual courtesies of library exchange. The writer is indebted to Professor F. A. Kingsbury for suggesting the problem and for advice and criticism during the progress of the study.

[2]G. W. Allport, A test for ascendance-submission, *J. Abn. & Soc. Psychol.*, 23, 1928, 118-136.

[3]M. Freyd, Introverts and extroverts, *Psychol. Rev.*, 31, 1924, 74-87.

[4]F. A. Moss and T. Hunt, Ability to get along with others, *Indust. Psychol.*, 1, 1926, 170-178.

[5]S. L. Pressey and O. R. Chambers, First revision of a group scale designed for investigating the emotions, with tentative norms, *J. Appl. Psychol.*, 4, 1920, 97-102.

[6]M. Otis, A study of suggestibility of children. *Arch. Psychol.*, 11, 1924, (no. 70), 67-93.

It is not easy to explain all the discrepancies between our group and those of the different authors. In the cases of the ascendance-submission and introversion-extraversion tests, the differences are not so notable. On Pressey's test of emotionality, where the discrepancy is the greatest, it may perhaps be explained in several ways: first, Pressey's norms were obtained about 10 years before ours, and social attitudes about what is right and wrong, pleasant and unpleasant, may have changed sufficiently to affect the scores on his test; secondly, Pressey's group was presumably from smaller cities and rural districts, whereas

TABLE I

COMPARISON OF MEDIAN SCORES OF COLLEGE STUDENTS ON FIVE PERSONALITY TESTS

Test	This Study		Authors' Norms	
	No. *Ss*	Median	No. *Ss*	Median
Allport A-S (men)	203	− 0.55	727	0.00
Allport A-S (women)	155	13.25	120	15.40
Allport A-S (total)	358		847	
Freyd I-E* (men)	203	−11.23	100	−11.41
Freyd I-E* (women)	155	−10.30	100	−11.12
Freyd I-E* (total)	358	−10.85		
Pressey X-O (men)	203	166.10	56	
Pressey X-O (women)	155	171.36	58	
Pressey X-O (total)	358	167.05	114	230
Moss Soc. Int. (men)	203	119.74		109
Moss Soc. Int. (women)	155	123.02		115
Moss Soc. Int. (total)	358	121.48	1217	111
Otis Sug. (men)	203	74.15		
Otis Sug. (women)	155	72.20		
Otis Sug. (total)	358	73.50	83	83.26

*For comparison see E. Heidbreder, Measuring introversion and extraversion, *J. Abn. & Soc. Psychol.*, 21, 1926, 120-138.

ours are preponderantly of urban background; thirdly, Pressey's norms were based on only 114 cases, whereas we have 358. On a test whose total score may range theoretically from zero to 500, and empirically from 45 to 339, 114 cases are scarcely enough to establish reliable norms.

On Moss's social intelligence test, the discrepancy may be partly accounted for by the difference in administering the test, since our *Ss* were allowed to fill in the blanks at home.

On the Otis test of suggestibility a very marked difference in the central tendency exists between our group and Otis's standardization group. No explanation seems available, and a search for other reports on the test yielded nothing.

In an effort to discover the interrelations among the personality traits as measured by the tests, coefficients of correlation were computed for each test with every other. The *r* are presented in Table II. It will be noticed that there is some difference between *r* for the same traits in men and in women, with the single exception of that between suggestibility and introversion-extraversion.

In all but two cases, the r between the tests for men are higher than those for women. Also, most of the r are extremely low, except the one between ascendance-submission and introversion-extraversion for men (−0.51), and the one between emotionality and introversion-extraversion for women (0.31). It must be admitted that all these r prove little, since little is known positively about the validity of the tests. Thus whatever findings may result from our investigation will always be subject to this limitation.

TABLE II

INTERCORRELATIONS BETWEEN THE FIVE PERSONALITY TESTS

Test	(A-S)	(I-E)	(X-O)	(S-I)
I-E (men)	−.51±.03			
I-E (women)	−.19±.05			
X-O (men)	−.17±.05	.20±.04		
X-O (women)	−.10±.05	.31±.05		
Soc. Int. (men)	.00±.05	.11±.05	−.21±.04	
Soc. Int. (women)	.16±.05	.01±.05	−.06±.05	
Sug. (men)	.15±.05	−.10±.05	−.20±.04	.22±.04
Sug. (women)	−.04±.05	−.07±.05	−.03±.05	.13±.05

Early personal history. The questionary on early personal history consists of 48 questions, grouped into 5 parts according to the type of instructions necessary and to the chronological reference of the questions.

Part I asked for information as to the number of siblings in S's family, his own position among them, and the number of step- or foster-brothers and sisters.

Part II asked for father's and mother's age at S's birth; in case either parent were dead at the time of answering the questionary, the age of S at the time of the death; in case the parents were divorced, S's age at that time; and the person with whom he lived afterwards; age at re-marriage of either parent; other adults living in the home before S was 6 yr. old; father's occupation; physical defects of S, their cause and the age at which they were acquired; time spent at kindergarten; age at beginning grade school; number of grades skipped or repeated; years needed to finish high school; number of teachers, and favorite and most disliked subjects in high school.

Part III referred to the period from birth to 7 yr. old. It asked whether the mother had been an active business or professional woman; how S had been disciplined by his father and by his mother; how much freedom his parents allowed him; what was the type of religious surroundings at the home; the condition of S's health, the predominating type of play, opportunities for companionship with children of his own age, and for travel.

Part IV referred to the period between 7 and 12 yr. of age. It dealt with the extent to which S read for pleasure, participated in games at school, attended religious meetings, and had companionship with children of his own age; the

nature of his relationship with such other children (*e.g.* whether admired or teased); the extent to which he partook of social amusements; the source of his spending money, and the kind of school he attended.

Part V referred to the period from 12 to 16 yr. of age. It dealt with the extent to which *S* indulged in reading outside of school requirements, and the nature of such reading; the extent of his participation in school games, and in religious activities; the extent of his acquaintances and friends of the same sex, and of the opposite sex; the nature of his relationship with schoolmates or playmates; the types of amusement most indulged in, and the extent to which *S* worked for pay.

In tabulating answers to the questionary it was deemed necessary to group certain ones into convenient categories, or classes. For example, the occupation of the father is grouped under: (1) executive, (2) professional, (3) merchant-salesman, (4) clerk-labor, and (5) farmer; the ages of the parents at time of *S*'s birth are grouped into: (1) under 25, (2) 26 to 30, (3) 31 to 35, and (4) 36 or over; and so on for other questions. After thus grouping the answers to certain questions, the total number of answer-variables used in tabulating the results was reduced to 208. The tabulation was based wholly on frequency, and no effort was made to evaluate or rate any of the answers in any other respect. A frequency distribution of the answers by all the *S*s to the complete questionary is not presented here because the table would contain facts which are mainly of sociological and educational rather than of psychological value.

Results

(*1*) *Personal history vs. ascendance-submission.* In considering the relationship between early personal history and personality traits our method was to trace from the differences in the personality traits to the corresponding differences in the early history variables. To do so we needed simply to differentiate the highest and lowest groups with reference to each personality trait, and to ascertain the corresponding group differences, if any, in the responses to the early personal history questionary. Thus, in showing the relationship between the early history and ascendance-submission, we selected the highest and lowest 20% of men on the A-S test, (repeating the process for women) and compared the frequencies of the responses of each group to each of the questions in the early personal history. Only those answer-variables whose total frequency was over 10 and for which one group was more than twice as large as the contrasted group were considered significant. Most promising for the differentiation of ascendance and submission are such facts in the early history as the following:

For ascendance.

Reading omnivorously for pleasure (men: 23 A, 13 S; women: 21 A, 12 S);
Participating in games at every opportunity (men: 18 A, 10 S; women: 18 A, 8 S);
Being admired by associates (men: 24 A, 4 S; women: 18 A, 4 S).

For submission.

Favoring languages in high school (men: 1 A, 9 S; women: 6 A, 16 S).
Having limited friendship with own sex (men: 1 A, 10 S; women: 2 A, 11 S).

(*2*) *Personal history vs. introversion-extraversion.* Inasmuch as the same test of introversion-extraversion and the same questionary on early personal history

were used for both sexes, we are able here to consider the relationship between the trait and the history jointly as well as separately for the two sexes.

The variables which seem particularly promising for differentiating introversion-extraversion are the following:

For introversion.
Having no, or just one or two, playmates (18 I, 3 E).
Indulging in social amusements only when urged (15 I, 3 E).
Practically never indulging in social amusements (10 I, 2 E).
Participating in games only when asked, or practically never (25 I, 2 E).
Having only a few intimate friends among own sex (20 I, 2 E).
Having practically no friends among opposite sex (25 I, 6 E).
Going to shows alone for recreation (23 I, 8 E).

For extraversion.
Having many playmates (9 I, 25 E).
Participating in games at every opportunity (10 I, 27 E).
Being admired by associates (6 I, 15 E).

The variables of particular value in differentiating I-E in women are:

"No consistent effort" in discipline by mother (10 I, 2 E).
"Few kinds" of religious participation (17 I, 5 E).

The source of spending money in early boyhood seemed an important indicator in differentiating I-E in men, as the following results show:

Worked at home for spending money (3 I, 10 E).
Had a regular allowance (5 I, 12 E).
Had an irregular allowance (21 I, 7 E).

(3) *Ascendance-submission vs. introversion-extraversion.* It will be recalled that by far the highest r in the table of intercorrelations among personality traits is that between ascendance-submission and introversion-extraversion for men, the r being -0.51. It will be interesting, therefore, to see how many and what early history variables function in common between I-E and A-S in the direction that supports this correlation. They are as follows:

father's age at *S*'s birth, 36 years or over (14 submissives, 16 introverts, 5 ascendants, 7 extraverts);
favoring languages in high school (9 submissives, 10 introverts, 1 ascendant, 3 extraverts);
participation in games at every opportunity between 7–12 yr. of age (10 submissives, 12 introverts, 18 ascendants, 20 extraverts), between 12–16 (15 ascendants, 17 extraverts, 4 submissives, 6 introverts);
admired by playmates in period from 7–12 yr. of age (18 ascendants, 16 extraverts, 8 submissives, 8 introverts), from 12–16 (24 ascendants, 18 extraverts, 4 submissives, 3 introverts);
only a few intimates of own sex in period from 12–16 yr. of age (10 submissives, 14 introverts, 1 ascendant, 2 extraverts);
no friends of opposite sex when in period from 12–16 yr. of age (16 submissives, 17 introverts, 7 ascendants, 4 extraverts);
finishing high school in less than 3 years (7 ascendants, 5 extraverts, 0 submissives, 1 introvert);
going to parties in period from 12–16 yr. of age (18 ascendants, 15 extraverts, 5 submissives, 6 introverts).

(4) *Personal history vs. social intelligence.* In considering the relationship between early personal history and social intelligence, as measured by the Moss

test, attention may be called to two points. First, it is questionable whether social intelligence is a personality trait in the same sense that the other four tests imply. Disregarding what an acceptable definition of social intelligence might be, it seems clear, from the content of the Moss test, that what it measures is a certain kind of knowledge or ability rather than a characteristic mode of behavior. Secondly, if the above statement is true, then a certain consistent relationship is to be expected between the test-score and early history, since social intelligence is a product of the social environment and training that constitute a large part of the early personal history. Thus any relation we may find between the history and the test-score would be no discovery, but can be considered merely as a confirmation of the effects of learning or habit formation. It is interesting, nevertheless, to list the early history variables on which those who scored high on Moss's social intelligence test seem to differ significantly from those who scored low.

Only 5 or 6 answer-variables seem to differentiate 'social intelligence' for both sexes in the same direction. It is difficult logically to account for this sex-difference. Furthermore, many of the answer-variables do not show as clean-cut differentiation as those obtained in the case of ascendence-submission or that of introversion-extraversion. The only variables to which especial attention need be directed are the following:

For men:

Fifteen of those having no friends among the opposite sex have a high score and 4 a low score in social intelligence; 11 of those studying art and music have a high score and 3 a low score in social intelligence; 7 of those participating in games at every opportunity tend to have a high score and 18 a low score.

For women:

Fifteen women who skipped 2 grades or more have a high score, 5 a low score; 15 who read omnivorously for pleasure have a high score and 4 a low score;

For both:

Seventeen men and women who never travelled have low scores, 7 high scores; and 17 men and women who played solitary games for recreation have high scores, and 4 have low scores.

(*5*) *Personal history vs. emotionality.* Only one answer-variable, namely "many kinds of religious participation," seems to be significant for emotionality as measured by the Pressey X-O Test. Twenty-four men whose religious atmosphere in childhood was conservative made a high emotionality score, and 12 a low one. Taking men and women together, 13 who "practically never" participated in religious activities during adolescence made high emotionality scores, while 22 made low ones.

(*6*) *Personal history vs. suggestibility.* Of the *S*s, one or both of whose parents were under 25 yr. of age at the time of *S*'s birth, 38 showed a high degree of suggestibility and 19 a low. The direction of this result is reversed for *S*s born of older parents. Of the *S*s who, between the ages of 7–12 yr., participated in games at every opportunity, 12 made high scores in suggestibility and 29 made low. Of those who, during the 7–12 yr. age-period, participated in games only when asked, 9 made high scores and 5 low; and of those who, during the 12–16 yr. age-period, participated only when asked, 17 made high scores and 7 low.

Summary and Conclusions

The major results of this study may be summarized as follows:

(1) The answer-variables most significant for ascendance are: reading omnivorously for pleasure, and participating in games at every opportunity. For submission: favoring languages in high school, reading very little for pleasure, and having limited companionship.

(2) Significant for introversion may be mentioned such items as having few playmates as a child, indulging in few social amusements, participating rarely in games at school, and having few or no friends. For extraversion: having many playmates, participating in games at every opportunity, and being admired by associates.

(3) Since the highest intercorrelation between the personality traits was found to be the one between ascendance-submission and introversion-extraversion for men, we noted what answer-variables were significant for differentiating both traits simultaneously. These were: father over 35 at *S*'s birth, *S*'s favoring languages in high school, participation in games at every opportunity, desire for admiration by playmates, lack of intimates of own sex and none of opposite sex during early adolescence, finishing high school in less than 3 years, and attendance at parties during early adolescence.

(4) In social intelligence, where one might expect *a priori* a high degree of relation between the early personal history and the test score, we actually obtained fewer significant answer-variables than in the cases of A-S or I-E.

(5) In studying the relationship between early personal history and emotionality, only one answer-variable applies to both sexes; those who participated in many kinds of religious activity in their adolescence are likely to score high on the Pressey test, and vice versa.

(6) As for suggestibility, especial attention is called to the question on parents' age, the answers to which showed that those *S*'s, both men and women, who were born to parents under 25 years of age had a tendency to be highly suggestible. Also, participation in games at every opportunity and in many kinds of religious activities seems connected with low suggestibility, whereas participating in games only when asked or urged seems to be connected with high suggestibility.

Within the limits of this study and with no claim to completeness or finality, our results seem to justify the following general conclusions:

(1) It appears possible by the method of group differences to discover in a person's early life significant facts in determining some of his later personality traits.

(2) The number and kind of such significant facts and the degree of their significance vary with the traits.

(3) The largest number and the most reliable of the significant facts in early personal history are found for predicting introversion-extraversion, the fewest and least reliable for emotionality.

(4) The results of this study have opened up a large number of specific problems suitable for further research concerning the relationship between certain facts in one's early personal history and certain of his personality traits.

FACTORS INFLUENCING VISUAL IMAGERY FOR LETTER GROUPS

By HENRY BOWERS, Normal School, Ottawa, Canada

Serious experimental difficulties involved in the study of imagery have hindered the investigation of this mental activity in its relationship with spelling, reading, and arithmetic. It seemed to the writer that a new approach to the study of visual-verbal imagery might be made through evaluation of the relative imaginal content of the experimental material.

PROBLEMS

The object of the present study was: (1) to determine if significant differences exist between single lower-case letters[1] in respect of the mean distinctness of their visual images in a group of *Ss*; and (2) to determine if significant differences exist between groups of lower-case letters in respect of the mean distinctness of their visual images. If the differences were found we hoped (3) to investigate the relative contributions of the distinctness of the visual images of specified letters or letter-combinations to that of larger letter groups.

METHOD AND PROCEDURE

The 26 letters of the alphabet were typed in lower-case form one underneath another on a strip of paper. Every one of the 183 *Ss*, having been given a strip, rated each letter on a 6-step scale. Values 0, 1, 2, 3, 4, 5 were arbitrarily assigned to each step on this scale. The group was divided randomly into two, and the mean estimate for each letter was found for each group. It was immediately obvious that enough reagents had been employed to give stability to the mean estimates of each sub-group. This is indicated mathematically by the Pearson *r* of 0.99 between the two sets of mean estimates. A similar procedure was followed in the case of lists of 2-letter groups, 3-letter groups, and 4-letter groups. In each of these cases 40 groups of letters were used. The 2-letter, 3-letter, and 4-letter groups were so constructed that it was possible to calculate the *r* between mean estimates for specified component groups and the whole letter-group; *e.g.* between the first two, or the last three letters of a 4-letter group and the whole group.

It was found (1) that small but very consistent differences exist between single lower-case letters in respect to the mean distinctness of their visual images in a group of *Ss*, (2) that substantial correspondence exists between mean estimates by groups of *Ss* for the visual imagery of 2-, 3-, or 4-letter combinations, and (3) that the visual imagery of letter groups is not wholly determined by the mean imagery of each constituent letter. It is also influenced by the imagery for sub-groups of letters. For example, the imagery of the first two (or the last two) letters in a 3-letter group is more potent in determining the estimate for the whole group than the three letters considered as individuals.

*Accepted for publication April 1, 1931.

[1]Twelve-point typewriter type was used.

Table I shows the letter-groups used in the experiment. Following each group is a number which refers to the relative distinctness of the visual images of the group. These numbers, of course, did not appear on the original material. It may be mentioned that except for columns (1) and (2), these numbers are not comparable from column to column, since different groups of students acted as *S*s. As indicated in the captions of the table, groups of *S*s numbering from 42 to 183 were used. These *S*s were Normal School students and form a comparatively homogeneous group.

After a preliminary discussion of visual imagery the following instructions were distributed with lists of numerals.

Please look closely at each of the following numbers. Then try to see them by means of your *mind's eye*. Some people do this more efficiently when their eyes are closed.

Would you say that—

S Your mental picture was as clear and distinct as the real numbers?
F Your mental picture was almost as clear and distinct as the original numbers? (Between S and P).
P Your mental picture was fairly clear and distinct as compared with the original numbers? (Between F and N).
N Your mental picture was quite blurred, vague and indistinct as compared with the original numbers? (Between P and H).
H Your mental picture had only the very slightest resemblance to the original numbers? Between N and K).
K You could not form a mental picture of the numbers?

Write the appropriate letter (S, F, P, N, H or K) in capitals at the left of the numbers.

Caution. Work slowly, carefully and conscientiously. Do not think that you are making a high score by claiming a very distinct mental picture, or that you are reducing your mark by claiming a vague and blurred one.

Results

The introspections on the numerals were treated as practice and the resulting data discarded. A brief discussion was again held, in which it appeared that negative after-images of the numerals appeared in a number of cases. Advice was given not to fixate the material too long, and to allow the after-images to fade out. The *S*s were required to form images of the same dimensions as the original figures, and to project them on an imagined white paper at the same distance from the eye as the given strip. An interesting observation was made at this point. One *S* naïvely complained about the numerals "jumping up from the paper" and "going into position." She was found to have a number-form. The letters K, H, N, P, F, and S were assigned arbitrary values, 0, 1, 2, 3, 4, 5, respectively. The mean of all the ratings for each letter or letter-group was calculated. These means are inserted in parentheses after the groups in the above lists. The probable errors of the mean claimed imagery for certain letter-groups (principally those of median imaginal content) were calculated. These data are given in Table II.

A random division of the *S*s into approximately equal groups was made. Coefficients of correlation between the mean estimates of these halves were computed. They are reported in Table III.

TABLE I

LETTER-GROUPS USED IN THE EXPERIMENT AND THE RELATIVE DISTINCTNESS OF THEIR VISUAL IMAGES

	No. of Subjects											
	182		183		125		83		92		42	
	(1)		(2)		(3)		(4)		(5)		(6)	
a	(2.9)	ab	(2.8)	abr	(2.7)	rab	(3.4)	abrt	(2.5)	trab	(2.7)	
b	(3.2)	ck	(2.7)	ckj	(2.5)	jck	(2.7)	ckjm	(2.0)	mjck	(2.2)	
c	(3.5)	jq	(2.2)	jqm	(2.1)	mjq	(2.3)	jqmi	(1.8)	imjq	(2.0)	
d	(2.9)	rl	(2.7)	rlz	(2.5)	zrl	(2.5)	rlza	(2.1)	azrl	(2.2)	
e	(3.2)	bs	(2.7)	bsk	(2.5)	kbs	)2.5)	bsky	(2.0)	ykbs	(2.0)	
f	(3.0)	ei	(3.0)	eif	(2.7)	fei	(3.1)	eifh	(2.2)	hfei	(2.2)	
g	(2.2)	gt	(2.1)	gtx	(2.1)	xgt	(2.1)	gtxd	(1.6)	dxgt	(2.1)	
h	(3.2)	hm	(2.6)	hmg	(2.3)	ghm	(2.3)	hmgn	(1.8)	nghm	(1.9)	
i	(3.5)	uy	(2.4)	uys	(2.4)	suy	(3.1)	uysl	(2.0)	lsuy	(2.4)	
j	(3.2)	br	(2.6)	brc	(2.7)	cbr	(2.9)	brco	(2.5)	ocbr	(2.7)	
k	(2.8)	dw	(2.4)	dwn	(2.5)	ndw	(2.5)	dwnv	(2.0)	vndw	(2.1)	
l	(3.7)	fn	(2.7)	fno	(2.8)	ofn	(3.0)	fnoj	(2.1)	jofn	(2.7)	
m	(2.8)	kz	(2.3)	kzl	(2.2)	lkz	(2.5)	kzlr	(1.8)	rlkz	(2.0)	
n	(3.2)	pj	(2.6)	pje	(2.3)	epj	(2.5)	pjex	(1.9)	xepj	(2.1)	
o	(3.9)	lh	(2.9)	lhu	(2.4)	ulh	(2.6)	lhuk	(2.0)	kulh	(2.3)	
p	(3.1)	xv	(2.7)	xvp	(2.4)	pxv	(2.6)	xvps	(2.0)	spxv	(2.1)	
q	(2.8)	td	(2.7)	tdb	(2.3)	btd	(2.6)	tdbe	(2.0)	ebtd	(2.0)	
r	(3.0)	oe	(3.1)	oev	(2.9)	voe	(3.5)	oevz	(2.3)	zvoe	(2.3)	
s	(3.1)	mg	(2.2)	mgi	(2.4)	img	(2.8)	mgiu	(1.8)	uimg	(2.0)	
t	(3.3)	if	(3.2)	ifd	(2.5)	dif	(3.3)	ifdq	(1.9)	qdif	(2.0)	
u	(3.0)	qc	(2.5)	qca	(2.4)	aqc	(2.7)	qcab	(2.2)	baqc	(2.2)	
v	(3.4)	nx	(2.5)	nxt	(2.6)	tnx	(2.5)	nxtw	(1.9)	wtnx	(1.9)	
w	(2.6)	vo	(3.1)	voq	(2.7)	qvo	(2.7)	voqg	(1.9)	gqvo	(1.8)	
x	(3.1)	wp	(2.3)	wpy	(2.0)	ywp	(2.0)	wpyc	(1.7)	cywp	(2.0)	
y	(2.8)	zu	(2.4)	zuw	(2.3)	wzu	(2.2)	zuwp	(1.8)	pwzu	(1.7)	
z	(3.1)	ya	(2.5)	yaq	(2.3)	qya	(2.2)	yaqf	(1.9)	fqya	(2.0)	
		qb	(2.4)	qbh	(2.2)	hqb	(2.2)	qbhx	(1.7)	xhqb	(1.9)	
		wf	(2.2)	wfz	(2.2)	zwf	(2.2)	wfzm	(1.7)	mzwf	(1.8)	
		sp	(2.8)	spk	(2.7)	ksp	(2.6)	spkt	(2.3)	tksp	(2.2)	
		rk	(2.4)	rkg	(2.1)	grk	(2.3)	rkgn	(1.9)	ngrk	(1.8)	
		bh	(2.5)	bhe	(2.7)	ebh	(2.8)	bhej	(2.0)	jebh	(2.4)	
		eu	(2.9)	eus	(3.1)	seu	(3.2)	eusi	(2.6)	iseu	(2.6)	
		mj	(2.5)	mjn	(2.4)	nmj	(2.5)	mjno	(2.2)	onmj	(2.5)	
		io	(3.2)	ioc	(3.1)	cio	(3.4)	iocb	(2.5)	bcio	(2.8)	
		yd	(2.6)	ydb	(1.9)	byd	(2.9)	ydba	(1.9)	abyd	(2.8)	
		xg	(1.9)	xga	(1.9)	axg	(2.3)	xgaz	(1.6)	zaxg	(1.9)	
		nf	(2.6)	nft	(2.4)	tnf	(2.5)	nftq	(1.9)	qtnf	(1.8)	
		cz	(2.7)	czp	(2.3)	pcz	(2.5)	czpl	(2.0)	lpcz	(2.1)	
		qw	(2.1)	qwd	(2.1)	dqw	(2.1)	qwdr	(1.8)	rdqw	(1.9)	
		la	(3.2)	lao	(3.0)	ola	(3.5)	laoh	(2.7)	hola	(3.2)	

TABLE II

PROBABLE ERRORS OF THE MEAN IMAGERY CLAIMED FOR CERTAIN LETTER-GROUPS

Column in Table I	Letter-Group		No. *Ss*	Mean Imagery Claimed	P.E.$_m$
1	a		182	2.90	.059
1	g	(lowest)	182	2.25	.061
1	s	(median)	182	3.10	.063
1	o	(highest)	182	3.90	.059
2	hm	(median)	183	2.65	.061
2	yd	(median)	183	2.65	.065
3	lhu	(median)	125	2.45	.069
3	nft	(median)	125	2.40	.070
4	qvo	(median)	83	2.75	.086
4	jck	(median)	83	2.75	.091
5	tdbe	(median)	92	2.05	.082
5	bhej	(median)	92	2.05	.078
6	tksp	(median)	42	2.20	.143
6	baqc	(median)	42	2.20	.138

TABLE III

RELIABILITIES OF MEAN ESTIMATES OF IMAGERY

Column Table I	No. Letters in Group	No. Groups in Column	No. *Ss*	r
1	1	26	91 and 91	0.99 ± .003
2	2	40	91 and 92	0.93 ± .01
3	3	40	66 and 59	0.86 ± .03
4	3	40	41 and 42	0.92 ± .02
5	4	40	46 and 46	0.85 ± .03
6	4	40	21 and 21	0.77 ± .04

TABLE IV

COEFFICIENTS OF CORRELATION BETWEEN MEAN ESTIMATES

	2-letter groups	3-letter groups		4-letter groups	
Letter	Col. (2) (183 & 183 *Ss*)	Col. (3) (125 & 183 *Ss*)	Col. (4) (83 & 183 *Ss*)	Col. (5) (92 & 183 *Ss*)	Col. (6) (42 & 183 *Ss*)
1st	.73 ± .05	.56 ± .07	.48 ± .08	.44 ± .09	.53 ± .08
2nd	.58 ± .07	.41 ± .09	.55 ± .08	.33 ± .10	.55 ± .08
3rd		.42 ± .09	.22 ± .11	.37 ± .09	.23 ± .10
4th				.50 ± .08	.19 ± .11
1st & 2nd		.77 ± .04		.66 ± .06	
1st & 3rd				.84 ± .03	
3rd & 4th			.72 ± .05		.47 ± .09
2nd, 3rd & 4th					.73 ± .05
Mean of 1st & 2nd	.85 ± .03				
Mean of 1st, 2nd & 3rd		.63 ± .06	.64 ± .06		
Mean of all				.74 ± .05	.67 ± .06

These substantial r indicate that the mean estimates of the whole groups of *Ss* must be very stable. In other words, the employment of still larger numbers would have little effect in disturbing the order of precedence of these letter-groups in respect of imaginal content.

Examination of the six columns of letters will show that data were now at hand for calculation of the following r: (a) between mean estimates for 2-, 3-, or 4-letter groups and those for the individual constituent letters; (b) between mean estimates for 3-letter groups and those for the first two or last two letters; (c) between mean estimates for 4-letter groups and those for the first two, first three, last two or last three letters; (d) between mean estimates for 2-, 3-, or 4-letter groups and the mean of the mean estimates for the constituent letters. These data are reported in Table IV.

One hundred and seventeen *Ss* made estimates on groups of 1, 2, 3, and 4 letters. The mean estimates for a single letter-group were respectively 3.05, 2.5, 2.4, and 2.25.

Conclusions

(1) Small but very consistent differences exist among single lower-case letters (12-point typewriter type) in respect of the mean distinctness of their visual images in a group of *Ss*. The order of precedence, from those of lowest to those of highest distinctness, is: g, w, (q, y), (k, m), (a, d), (r, f, u), (x, s, z, p), (b, e, h, n, j), t, v, (i, c), l, o.

(2) Groups of 2, 3, and 4 letters may be arranged in series according to the distinctness of their visual images. The following series of 2-letter groups is in order of precedence, the first named being those with least distinct visual images: xg, (qw, gt), (mg, wf, jq), (kz, wp), (rk, uy, qb, zu, dw), (qc, bh, mj, ya, nx), (pj, br, nf, hm, yd), (cz, fn, xv, td, bs, ck, rl), (ab, sp), (eu, lh), ei, (vo, oe), (la, if, io).

(3) The distinctness of the visual images of 2-letter groups is contributed to by the mean distinctness of the two letters, the distinctness of the first, and the distinctness of the second. The order of magnitude of contribution is indicated.

(4) The first and second letters of a 3-letter group appear to be of equal importance in influencing the distinctness of the visual image of the group. There is some evidence that the third letter has rather less weight. The mean of the 3 letters has greater influence than the 3 letters considered singly, but has less weight than the sub-groups, 'first 2 letters,' or 'last 2 letters.' No difference was detected between the contribution of the latter.

(5) The sub-groups 'first 3 letters,' and 'last 3 letters' are of significantly greater importance in determining the distinctness of the visual image of a 4-letter group than the mean distinctness of the 4 letters. In order of importance are 'first 3 letters' (or 'last 3 letters'), mean of the 4 letters, 'first 2 letters' (or 'last 2 letters'), first or second letter, third or fourth letter. The order of the alternatives has not been established.

(6) The feasibility of evaluating verbal material in respect to the distinctness of visual images of it is established, and this is in accordance with conclusions previously arrived at in connection with concrete visual, auditory, and kinaesthetic imagery.[2]

[2]H. Bowers, Memory and mental imagery: An experimental study, *Brit. J. Psychol.*, 21, 1931, 271-282. See also Constancy of imaginal content, *J. Educ. Psychol.*, 20, 295-298; Studies in visual imagery, this Journal, 43, 1931, 216-229.

THE RELATION OF BRIGHT AND DULL PRESSURE TO AFFECTIVITY

By Paul Thomas Young, University of Illinois

In a recent number of this Journal[1] William A. Hunt described an experiment under the above title. Hunt's study rested upon an investigation by J. P. Nafe[2] and a critical experiment by the writer.[3]

Hunt exposed colored papers of various hues and brightnesses through a 5 in. aperture in a cream gray screen. His *Os* were at first instructed as follows:

I shall present various colored stimuli. You are to attend to the general experience involved in observing them, and to state whether this experience involves a 'bright pressure' or a 'dull pressure.' These pressures are qualitatively similar to those ordinarily elicited by cutaneous stimulation, but are in no way identical with them.

'Bright pressure' has been described as tactual bright pressure or a touch quality lying between bright pressure and tickle. It has expansive qualities and has been called radial, being more prominent at the edges than in the center. It is bright, buoyant, and has a diffuse bodily feel. The whole experience is diffuse and volumic, and is patterned, with the patterns varying from one experience to another. The following adjectives have been used in describing it: bright, sparkling, brilliant, active, effervescent, like tickle, dancing, shimmering, misty, mild, vaporous, light, airy, fluffy, ethereal, smooth, soft, welling, spreading, like expansiveness of the body.

'Dull pressure' has been described as dull pressure or a quality lying near dull pressure, or between dull pressure and strain. It is not extended. It has qualities of constriction, and has been called radial, but more prominent at the center than at the edges. While it is diffuse and volumic, it is less so than bright pressure and is duller and less bright. It has been called dull, massive, drab, dead, rigid, less lively, somber, inert, stiff, gloomy, more dense, heavy, sinking, leaden, thick, cold, rough, grating, insistent, condensed, like bodily contraction.

This instruction was used to *set* the *Os* for bright and dull pressure. The next week the same colored papers were presented to the *Os* with a different instruction. Apart from instruction the experimental conditions and procedure were unchanged. The second instruction follows:

I shall present various colored stimuli, and you are to judge whether they are pleasant, unpleasant, or indifferent.

A week later Hunt repeated the series of pressure judgments and the following week he repeated the series of affective judgments. The total results were treated quantitatively by assigning numerical values; to bright pressure was assigned the value +1, to indifference 0, and to dull pressure −1. Similarly pleasantness

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[1] W. A. Hunt, The relation of bright and dull pressure to affectivity, this Journal, 43, 1931, 87-92.

[2] J. P. Nafe, An experimental study of the affective qualities, *ibid.*, 35, 1924, 507-544.

[3] P. T. Young, The 'trained' observer in affective psychology, *ibid.*, 38, 1927, 175-185.

was assigned the value +1, indifference 0, and unpleasantness −1. These values were then summed to determine the degree to which a particular color elicited bright pressure, and the degree to which it elicited P.

The degree to which colors elicited bright pressure was then correlated with their P by the product-moment formula. The coefficients of correlation ranged from 0.72 to 0.99 and their P.E. from 0.003 to 0.08. When corrected for attenuation the coefficients approach +1.00.

Hunt concluded: "These results obviously lend support to Nafe's thesis. They do not, however, prove it."

A group experiment. The following experiment was performed at the University of Illinois with words taken from Hunt's first instruction. It should be noted that Hunt took from Nafe the terms which presumably describe bright and dull pressure; and that Nafe derived these terms directly from the reports of his *Os*. Forty-three descriptive terms were taken from Hunt's instruction, arranged in a chance order and then used as stimulus-words in an affective-association test.[4]

Mimeographed data sheets were distributed to 242 students in the writer's elementary class in psychology. The following instruction was then read slowly:

I will read some words, one at a time. If a word suggests pleasantness, put down P in the space provided; if it suggests unpleasantness, put down U; if you are not sure, guess.

Do not deliberate, argue or reflect; be passive, matter-of-fact, uncritical. Let the word have its effect and put down the first feeling suggested to you.

Every precaution was taken to gain the coöperation of the group and to maintain conditions satisfactory for a group test.

Fourteen records were incomplete, so the present report is based upon 228 cases, 138 men and 90 women. The *Ss* were sophomores and upper classmen.

From the records the writer tabulated the frequency with which a word suggested P. Table I gives the words arranged in order from most to least P. Since in every case the record was either P or U, the frequency for U is 228 minus the given frequency for P.

At first men and women were tabulated separately,[5] but, since no significant sex-differences were found, the results have been treated collectively. By Spearman's method of rank-differences a positive correlation of 0.96 was found between men and women; $P.E._r$ was 0.008.

Since there were 228 *Ss* half of this number, or 114, will be taken as a convenient indifference point. There are 17 terms (Table I, first column) on the

[4]The chance order follows: *dull, mild, shimmering, airy, brilliant, dense, lively, smooth, soft, condensed, somber, stiff, like expansiveness of the body, like bodily contraction, sinking, light, rigid, rough, ethereal, grating, inert, misty, like tickle, insistent, welling, active, effervescent, spreading, sparkling, heavy, vaporous, cold, fluffy, massive, dead, drab, gloomy, thick, bright, leaden, dancing, buoyant, diffuse.*

In the above list Hunt's *more dense* was changed to *dense;* and *less lively* to *lively*. The word *effervescent* was inadvertently pronounced *effervescence*. Otherwise the present words and Hunt's are the same.

Incidentally there was mild laughter when the words *stiff* and *dead* were pronounced.

[5]The *Ss* indicated sex on the data sheets but were not asked to give their names.

P side of indifference and 26 terms (Table I, last two columns) on the U side. In other words, the total list is slightly more suggestive of U than of P.

A study of the list in relation to the words in Hunt's instruction shows that *all* P terms (*i.e.* those with a frequency greater than 114) without exception are on the 'bright pressure' list. The U terms (*i.e.* those with a frequency less than 114) contain *all* of Hunt's words descriptive of 'dull pressure' and in addition 6 qualifiers which belong to 'bright pressure.' The 6 U terms which are on Hunt's 'bright pressure' list are all very near to indifference: spreading, welling, diffuse,[6] vaporous, misty, like expansiveness of the body.

TABLE I

FREQUENCY OF ASSOCIATION OF P WITH THE STIMULUS-WORDS LISTED
100% = 228.

lively	224	spreading	109	rough	18
sparkling	224	massive	105	sinking	14
dancing	221	welling	104	grating	13
active	219	diffuse	91	heavy	12
light	219	vaporous	88	stiff	11
smooth	216	misty	86	dense	10
bright	214	condensed	79	leaden	10
bouyant	211	somber	65	drab	8
brilliant	211	like expansiveness		gloomy	8
airy	206	of the body	60	dead	7
fluffy	204	like bodily		dull	6
mild	199	contraction	52		
soft	188	thick	39		
effervescence	168	rigid	38		
ethereal	165	insistent	33		
shimmering	154	inert	32		
like tickle	130	cold	26		

In general the result is exceedingly clear-cut—more so than the writer had dared to expect. The words *descriptive* of bright pressure suggest P and the words *descriptive* of dull pressure suggest U; some words (near indifference) are relatively nondescript.

Immediately following the 43 stimulus-words on our list another instruction was read to the group:

I will read a list of terms. Do not write anything until I have read the entire list. Then, if the total list suggests P, write P; if it suggests U, write U.

Two lists were read: first the word-list descriptive of 'bright pressure,' and then the list descriptive of 'dull pressure.' Both lists were taken from Hunt's instruction and read without change. The 'bright pressure' list began: "bright, sparkling, brilliant, active . . . ." The 'dull pressure' list began: "dull, massive, drab, dead . . . ."

[6]Bright pressure is said to be more diffuse than dull pressure, but the word *diffuse* may apply to either. Strictly speaking the word is not on Hunt's list. See the instruction.

The result was decisive and scarcely needs any comment:

	Men	Women	Total
P suggested by 'bright pressure' list	131	90	221
U " " " "	1	2	3
P suggested by 'dull pressure' list	1	2	3
U " " " "	131	90	221

There were 3 persons who for some reason or other gave just the reverse of what we had expected.

It is fair to conclude that the terms alleged to be descriptive of 'bright pressure' are highly suggestive of P and those descriptive of 'dull pressure' are suggestive of U.

Interpretation. There are many words in the English language which suggest P and U feelings; in addition to the ones already considered are[7] *enjoyable, pretty, nice, sweet, rich, vibrant, corking, cheerful, happy,* etc.; and *horrid, terrible, draggy, sad, nauseating, uncomfortable,* etc. An affective-association test is a simple means of determining the degree to which a given word suggests P or U. In this connection the writer is reminded of experiments by Titchener, Hayes, and Foster and Roese[8] upon the Wundtian theory of feeling. In their work, curves of preference gained by the method of paired comparisons as well as introspective reports indicated that certain Wundtian affections are related to P and U. 'Calm' and 'relaxation' are apt to be P and 'strain' and 'depression' are apt to be U.[9]

It seems to the writer that many of the words used by Hunt do not *describe* a pressure-like experience. What, for example, is a 'gloomy, drab, dead' pressure? And what a 'brilliant, active, dancing' pressure? Opinions may differ as to how far these words *describe* pressure; but there is no doubt that the 'bright pressure' words *suggest* P and the 'dull pressure' words *suggest* U.

The present result is not surprising when one recalls, (1) that the 'bright pressure' words were used originally in an attempt to characterize P experiences and the 'dull pressure' words were used to characterize U; and (2) that Hunt's *Os* were limited by instruction to two categories of report, namely, pressure (bright or dull) and affection (P or U).

That suggestion is involved is indicated by one of Hunt's control experiments. Three "wholly naïve observers" made a single series of pressure judgments and after a month a single series of affective judgments. Correlations between the two series were 0.82, 0.92, and 0.88, with P.E. respectively of 0.05, 0.02, and 0.03. Obviously in this case prolonged psychological education and training are out of the question. Previously the writer attempted to confirm Nafe's result by giving three *Os* ample practice under conditions which precluded suggestion; but he failed to duplicate Nafe's results.[10] In Hunt's control experiment the effectiveness of verbal suggestion in the absence of practice is demonstrated. In determining a result suggestion, therefore, is more effective than practice (repetition of observation).

[7]Some of these examples are taken from O. D. Anderson, An experimental study of observational attitudes, this JOURNAL, 42, 1930, 354.

[8]For references, see W. S. Foster and K. Roese, The tridimensional theory of feeling from the standpoint of typical experiences, this JOURNAL, 27, 1916, 157.

[9]Cf. the writer's results, Pleasantness and unpleasantness in relation to organic response, *ibid.*, 32, 1921, 40.

[10]*Op. cit.*, footnote 3.

Can the pressure-affection correlation be demonstrated by methods which rigidly exclude suggestion?[11]

Conclusion. Hunt's high correlations between pressure and affection probably depend upon verbal suggestions given to the *Os* in an instruction. Bright pressure was described in terms highly suggestive of P and dull pressure in terms highly suggestive of U. His experiment shows how easily verbal suggestion can determine a result. Again we are in doubt whether Nafe's thesis can be demonstrated without suggesting it in the instructions or otherwise.

[11]It would be unfortunate if the present discussion obscured genuine problems. Nafe and others have concluded that P and U are directly observable experiences similar to pressure; that the difference between P and U pressures may best be described by the words *bright* and *dull;* and that there is some evidence that bright pressure is localized in the region of the chest and dull pressure in the region of the abdomen. These statements need to be tested by a method in which the instructions do not suggest the result.

INTENSITY OF STIMULATION AND THE AMPLITUDE AND LATENCY OF THE LID-REFLEX

By Helen Peak, Yale University

This study reports briefly three preliminary generalizations regarding the effect of the intensity of stimulation on the reflex response of the lid to a sudden noise: (1) the amplitude of the lid-reflex tends to increase in direct proportion to the logarithm of the stimulus intensity; (2) the increment of increase of response with increased intensity of stimulation is less under a state of relaxation than under a relatively tense state of the reactor; (3) reflex latency shows a slight decrease with increasing intensity of stimulation. When comparing responses of the same amplitude, however, this difference in time disappeared in all but one *S*.

These facts are incidental to an experiment reported elsewhere,[1] where the description of the apparatus and experimental procedure may be found. To summarize the experiment briefly, we note that the lid-reflex, in response to four intensities of auditory stimulation, was elicited on the background of psychophysiological sets evoked by three types of instruction. In the first set *S* was instructed to relax, in the second, to wink voluntarily just as quickly as possible, and in the third, to press a key voluntarily with the finger and just as quickly as possible. Five *S*s were given about 900 stimulations each. The lid-response was recorded photographically. The stimulating apparatus was calibrated so that the relative intensities could be identified and measured. The relative values derived from this calibration are given in Table I alongside letters which designate the different intensities (A is the lowest intensity). These values are purely relative and cannot be interpreted in absolute terms. They refer to the squares of readings on a photographic film which are direct functions of the displacement of the sounding board. Since the frequency values of these displacements are not known, the absolute intensities cannot be derived. The intensities were arranged in the experimental series so that each occurred once in every four trials of a practice series of twenty-four trials. No intensity succeeded itself.

TABLE I

Average Amplitudes for All Subjects by Sets and Intensity

Intensity		Relaxation (mm.)	Lid-voluntary (mm.)	Finger-voluntary (mm.)
(A)	22.09	8.9	11.4	11.2
(B)	144	13.2	17.3	15.9
(C)	702.25	14.4	19.7	19.5
(D)	2218.41	17.2	23.8	23.0

*Accepted for publication April 1, 1931.

[1] H. Peak, Modification of the lid-reflex by voluntarily induced sets, *Psychol. Monog.*, 42, 1931, (no. 188), 68.

Curves of the form $y = k \log_{10} x + c$ have been fitted to the empirical points of Table I by the method of least squares.[2]

Amplitude. Fig. 1 shows the relationship between the intensity of stimulation and the amplitude of reflex response. In the inset the same data for amplitude are plotted against the logarithm of the stimulus intensity. The actual equations from which the curves were plotted are: (1) relaxation, y (amplitude) = 3.8907 log x + 4.035; (2) lid-voluntary, y = 5.902 log x + 3.762; (3) finger-voluntary, y = 5.832 log x +5.23.

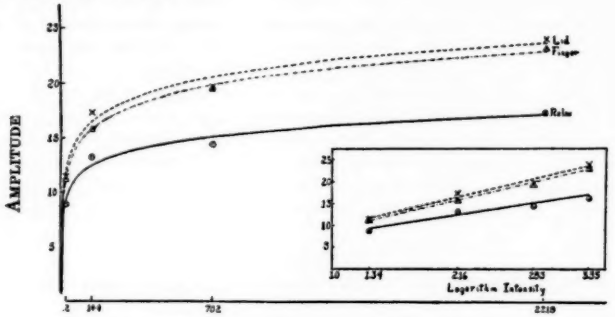

FIG. 1. THE RELATION OF AMPLITUDE OF RESPONSE TO INTENSITY OF STIMULATION

Amplitude values are represented on the y-axis, intensities on the x-axis. In the graph on the left-hand side of the figure, y-values are plotted against x-values; in the other graph, y-values are plotted against log x-values. For both graphs the empirical points are indicated by circles for the relaxation data, crosses for the lid-voluntary, and triangles for the finger-voluntary. The curves have been fitted to the data by the method of least squares.

In each set the empirical points approach close to the lines of best fit through these points so that the lines may be taken to represent the general tendency of the data. It is indicated, therefore, that the reflex response increases in direct proportion to the logarithm of the stimulus intensity, which is the relationship expressed in the Weber-Fechner law. These facts are probably observed best from the semi-log plot in the inset of Fig. 1, particularly the relation of the curves to the empirical points. An increase in amplitude of the reflex with increase in intensity of stimulation has been noted by numerous investigators, though the nature of the relationship has been variously stated and certain limiting conditions, under which the increase does not occur, have been reported.

The slope of the lines is different for the three sets; the greatest difference appears between the voluntary sets and relaxation. The values for the two

[2]The following formulae were used:

$$k = [(n\Sigma y)(\log x) - (\Sigma y)(\Sigma \log x)]/[(n\Sigma \log^2 x) - (\Sigma \log x)^2]$$

$$c = [(\Sigma y) - (k)(\Sigma \log x)]/n.$$

I am indebted to Dr. E. R. Hilgard for the derivation of these formulae.

voluntary sets differ only slightly. Thus the same added unit of physical intensity produces a greater increase in response under voluntary sets than under relaxation.

The problem of the relation of these lines at the threshold of response (when $y = 0$) suggests itself. When continued to the x-axis, the relaxation curve meets it at 0.092; the lid-voluntary, at 0.23; the finger-voluntary, at 0.28. These values are very close together, though they suggest a slightly higher threshold for the reflex under voluntary sets than under relaxation. Whether or not this is true depends, of course, on the constancy of the relationship found here between amplitude of response and intensity of stimulation. The evidence from other types of behavior indicates that such a relationship is likely to change near the threshold. A study of reflex thresholds under different sets, as well as the shape of the curves as the threshold is approached, is indicated as an important experimental problem.

TABLE II

AVERAGE LATENT TIMES IN σ FOR EVERY INTENSITY

Subject	Intensity (A)	(B)	(C)	(D)
1	48.3	46.4	46.2	46.0
2	40.4	38.8	38.4	37.8
3	40.3	38.3	38.1	36.6
4	40.5	38.8	39.4	38.1
5	38.2	36.3	37.7	37.4

Time. Numerous references to the relation between intensity of stimulation and the time of reflex response are to be found in the literature. Disagreement obtains, however, even among the studies which have employed more or less similar techniques. We have no conclusive data on the problem but wish to discuss briefly the error in determining latency which arises from the difficulty of reading time on responses of low amplitude.

We found, when examining the assembled time values of the lid-reflex for all *S*s and sets, that there was a slight decrease in latency of responses to greater intensities of stimulation. This was not consistently true from intensity to intensity, but in a comparison of the average time of response to the lowest intensity with the average time to the highest intensity, the highest intensity group showed the shortest time in 93% of the comparisons. Table II contains the composite average times of all sets for each *S* for each intensity of stimulation. Though the differences are small here, their direction is clear and responses to the low intensity A all show longer latencies than those to any of the higher intensities.

In order to determine whether this small difference was affected by the fact that the large intensities produced consistently larger amplitudes of response, which might have been read erroneously at shorter times, we decided to

compare the time of response to the different intensities in reactions of the same amplitude, thus making the amplitude factor constant. Reactions were placed in groups according to their amplitudes (group intervals of 3 mm.) and the average latency value for each group was determined. The averages of responses in the same amplitude group were then compared and it was found that the latency of response to the smallest intensity was longer than that to the largest only 47.5% of the time instead of 93% of the time, as it was before amplitude was 'partialled out.'

TABLE III

AVERAGE LATENCY IN σ FOR EVERY INTENSITY
(Amplitude constant)

	Intensity			
Subject	(A)	(B)	(C)	(D)
1	46.4	46.2	47.5	48.0
2	38.0	37.8	37.9	39.8
3	40.5	38.1	38.7	36.2
4	38.4	38.1	38.7	38.8
5	37.2	37.2	40.5	38.2

Table III gives data comparable to those of Table II. Here we have the composite means derived by averaging the means of each amplitude group. In each of these averages the amplitude factor has the same weight. It is clear from Table III that, with amplitude held constant, time differences disappear for all except *S* 4, who retains about the same relative values for different intensities. The factor of amplitude is thus shown to be related to latency. That the error of reading small amplitude responses may be responsible for this fact seems likely, but it should be remembered that a real relationship may exist between large amplitude and short latency which is not due to such a technical error. Whatever the cause of the correlation may prove to be, the empirical fact is established for the present data, and it should be considered in all cases where amplitude changes concomitantly with latency.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF NORTHWESTERN UNIVERSITY

I. Muscular Tension and Irritability

By G. L. Freeman and E. Taylor Katzoff

The relationship between emotional behavior and muscular tension has been mentioned in several papers of recent date. Two investigators, Johnson[1] and Duffy,[2] have produced evidence favoring the view that individual differences in emotionality are reflected by corresponding changes in the tension of skeletal muscle. Using the pressure exerted upon hand dynamometers as an indicator of tension, Duffy obtained records from nursery children during discrimination and tapping tests. The children were then rated by teachers and physicians for excitability (emotional stability) and school adjustment. Individual tension and excitability scores gave a coefficient of correlation of 0.52, P.E. 0.13. Tension and ratings for school adjustment gave a correlation of 0.58. While it is obvious that changes in muscular tension occur in connection with practically every mental state, the correlation of tension-increments during work with ratings for temperamental differences deserves critical study.

The purpose of the present report is to extend and amplify Duffy's observations.[3] Our specific problem was to record the changes in tension of adult *Ss* during experimentally produced irritations and to relate these changes with some objective measure of their emotional irritability in connection with situations of everyday life. Emotional irritability is here defined as the tendency to be annoyed, disturbed, or stirred-up by external occurrences. The general advantage of test over rating procedures led us to accept the Cason Common Annoyance Test as an indicator of what we have called emotional irritability in everyday situations.[4]

Experimental Procedure

The *Ss* were 50 college students ranging in age from 18 to 24 years. Each served for one session only. *S* was brought into a partially darkened experimental room and seated before a screen. In order to keep the purpose of the experiment from being too obvious, the following misleading information was given.

*Accepted for publication June 15, 1932.

[1]Buford Johnson, Changes in muscular tension in coördinated hand movements, *J. Exper. Psychol.*, 11, 1928, 334.

[2]Elizabeth Duffy, Tensions and emotional factors in reaction, *Genet. Psychol. Monog.*, 7, 1930, 65; The measurement of muscular tension as a technique for the study of emotional tendencies, this Journal, 44, 1932, 146-162.

[3]Our thanks are due to Miss Dorothy Black, Miss Eleanor Agnes, Miss Marjory Koch and Mr. Emanual Neilson for valuable assistance in the conduct of this experiment.

[4]H. Cason, An annoyance test and some research problems, *J. Abnor. & Soc. Psychol.*, 25, 1930, 224-236.

The purpose of this experiment is to measure changes in perspiration during reading. Seat yourself comfortably with your head against the rest. Take this bulb and hold it firmly in your hand; on no account are you to drop or release it. When the reading light is turned on, you will find a series of humorous articles which you are to read. Begin reading aloud when I say go and continue reading until you come to the end of the selection.

The bulb was connected with a sensitive tambour, writing on smoked paper. Since the air column was placed under pressure by closing the hand over the bulb, both the strengthening or weakening of the grip could be recorded.

S was allowed about 5 min. in which to become accustomed to the experimental situation. A record of the pressure upon the bulb was taken during this entire period, in order to establish a baseline from which subsequent changes could be measured. Six selections were read during the experimental period, and a single irritating stimulus was introduced with each passage, after *S* had become engrossed in the reading. The following stimuli were used in random order: (1) flickering of the reading light; (2) quick removal of support at back of *S*'s head; (3) falling of a large brass weight near *S*'s left thigh;[5] (4) snapping of a large mousetrap; (5) strong upward current of air, driven by electric fan near feet of *S*; (6) persistent filing and scratching on a screen.

Many of the *S*s reported that these stimuli produced irritations with a high degree of emotional content. They were somewhat at a loss to know just why they had been introduced; but the only definite cue to the purpose of the experiment was supplied *after* the session, when *S* was asked to rate himself on the Annoyance Test.

Auxiliary records of quadriceps tonus. Because pressure upon a bulb is a very crude indicator of tension and because it may involve voluntary increments in tension as well as involuntary changes,[6] auxiliary records of quadriceps tonus were obtained for 10 of the *S*s. Tonus was photographically recorded by the method of tendon deformation, as previously described.[7] These records showed a tendency to correlate with the changes in pressure on the bulb.

Scoring of the kymograph records. Changes in the pressure upon the bulb during and following the introduction of 'irritating' stimuli were reckoned in mm. of height from an arbitrary baseline, established at the midpoint of a 5-min. preëxperimental tension curve. Three measurements were taken of the response to each of the six stimuli; these were, (1) maximal height, or depression, in the curve, (2) height 5 sec. before maximal, (3) height 5 sec. after maximal. These three measures were then averaged and the six averages combined to give the composite tension score for each subject.

Results

The results of this experiment may be presented briefly in the correlation found to exist between individual tension scores and subjective ratings for annoy-

[5]This ball was arranged so that it would fall from a shelf outside *S*'s range of direct vision. An invisible wire prevented the ball from descending beyond a certain height.

[6]We are making the assumption that the tensions dealt with in this experiment were largely of an involuntary nature.

[7]G. L. Freeman, The measurement of tonus by deformation of the tendon, this Journal, 43, 1930, 581-592.

ance and irritability. They indicate that the person whose tension increases during irritating experimental situations will also rate such items as the following as being extremely annoying.[8]

To hear a person talking in a shrill voice.
To see a person picking his nose.
To see public love-making.
To hear water dripping from a faucet.
To have to kiss a relation I do not want to.
To see a person at table spitting out food.
To have a person look over my shoulder and read the book I am reading.

Because we were mainly interested in the *position* of individual tension and annoyance scores, independent of the *size* of the score, the coefficient of correlation was obtained by the rank-difference method. It was found that $\rho = +0.49$ (P.E.$_\rho$ = 0.07). This compares favorably with the figures reported by Duffy and indicates the tendency for tension to be as positively correlated with irritability in adults as it is in children. The fact that the coefficient is not any larger does not seem to us to depend upon the reliability of our sample as upon the methods employed to test for irritability and tension. From the standpoint of the distribution of scores, the measures of irritability are about as those of tension. The average annoyance score for our group is 18.5, m.v. 3.3. The average tension score is 12.7 mm., m.v. 5.7.

The validity of the tension scores is dependent upon two factors. For one thing, any pressure system is at best a very crude indicator of tension; in the 10 cases, where the more delicate technique of tendon deformation was used concomitantly, the coefficient of correlation between the two indicators was only 0.22.[9] In the second place, individual tension scores would have to be obtained upon a number of different days and under a variety of conditions before one could deal with the problem of individual differences very thoroughly. Duffy's work has a certain advantage over ours in this respect. She used 18 *S*s in the same situation for several sessions. This arrangement did not seem feasible in our experiment. The advantages of our study lie in the fact that we used a greater number of cases (50), and sought to arouse definite emotional reactions, whereas the occurrence of such reactions in Duffy's experiment was only incidental.

Discussion

Our results are further evidence that the differences in tension between individuals is not a fortuitous matter, but a highly significant one. *S*s who are tense and highly excitable in experimental situations, also tend to rate themselves high with regard to common annoyances. Annoyance may depend upon a number of things. Among those most commonly listed we find such expressions as 'excitability,' 'emotional maturity,' 'instability,' 'poor adjustment,' and 'irritability.' We have chosen to use the last term to describe the subjective factor

[8]To those who are unfamiliar with the Annoyance Test, we might add that *S* can give one of four ratings to each statement (3) extremely annoying, (2) moderately annoying, (1) slightly annoying, (0) not annoying; x indicates that *S* is unfamiliar with situation.

[9]This would be due partially to the differential relaxation between various members of the body. We recall that Duffy (*op. cit.*, 157) found a low correlation between the tension of both hands.

underlying most annoyances. In general, it is the tendency to be easily aroused and stirred-up, to respond in a somewhat irrational manner and to react emotionally as well as perceptually to external occurrences.

Why should one who is tense become more easily irritated than a person who is habitually more relaxed? The question is not an easy one to answer. We have very little in the way of valid objective differentia between emotions and other mental states. The most pertinent fact which we can bring to bear upon the problem is that the organism is *more highly aroused* in the so-called emotional state. Emotional arousal is measured by some indicator of organic activity. Muscular tension is one of these. The previous question might now be phrased somewhat differently: How can the degree of excitation already present in the organism determine how it reacts to a given external situation? If one accepts the principle that the impulses coming into the central nervous system from the proprioceptors act to lower thresholds of nervous irritability to all stimuli, we have the beginnings of an explanation.[10] If some individuals show themselves to be more easily annoyed and irritated than others, that tendency may be due, in part at least, to a neuro-muscular mechanism which is habitually geared to high pitch. Clinicians and others interested in the diagnosis of emotional tendencies do well to include some measure of involuntary muscular tension in their round of tests.

[10]We do not propose to discuss this matter in detail here. Cf. G. L. Freeman, Mental activity and the muscular processes, *Psychol. Rev.*, 38, 1931, 428-449, for a suggestive account.

RELATION OF SPEED OF REACTION TO INTELLIGENCE

By L. F. Beck, Brown University

Speed of reaction has frequently been regarded as a contributing factor in intelligence. Its significance is not, however, clearly understood; and the results of experiments made upon this topic have been ambiguous. McFarland, for example, in a summary of the literature, noted the contradictory nature of the results, but concluded nevertheless that there was "a vital relationship between rate and mental ability."[1] More recently Beck reviewed the field, and he came to the conclusion that speed was only of minor, if of any, importance in intelligence.[2]

The object of the present study is, therefore, to obtain further data upon the relation of speed and intelligence by determining specifically the correlation between speed in (1) simple reaction, (2) serial reaction, (3) reading, and the scores in an intelligence test.

Method and Procedure

Simple reactions. A Dunlap chronoscope was employed for measuring the simple reaction-times.[3] Every *S* was instructed to press a telegraph key with his right hand when he observed any movement of the pointer on the dial of the chronoscope. A preparatory stimulus, "ready," was given from 1½–3 sec. prior to the engaging of the recording mechanism of the chronoscope.

Serial reactions. For the measurement of serial reaction, the Seashore serial discrimeter was used.[4] This apparatus calls for the successive activation of the four fingers of the right hand in response to four numbers which are presented visually in a random succession.

Reading test. The reading test was constructed from magazine articles. In order that the *S*s could easily comprehend the material and still not be able to skim and to slight the reading, articles were selected whose content was comparatively simple but whose construction was quite difficult. The time permitted the *S*s for reading was 2 min. At the end of that interval the *S*s were given 24 short questions, every one capable of being answered in a single word or short phrase, the results of which were used as a measure of their rate of comprehensive reading.

Procedure. The testing program was divided into four series as follows: (a) 5 serial reactions followed by 25 simple reactions; (b) 5 serial reactions, 25 simple reactions, half of the reading test, and half of the comprehensive questions; (c) the same as (b); and (d) the same as (a). Thus in all 20 serial reactions, 100 simple reactions, and 2 scores of silent reading were obtained for every *S*.

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[1] R. A. McFarland, The rôle of speed in mental ability, *Psychol. Bull.*, 25, 1928, 610.

[2] L. F. Beck, The rôle of speed in intelligence, *ibid.*, (forthcoming).

[3] K. Dunlap, The Johns Hopkins chronoscope, *J. Exper. Psychol.*, 2, 1917, 249-252.

[4] R. H. Seashore, Techniques for measuring serial action, *ibid.*, 11, 1928, 45-55.

Thirty *Ss*, 15 men and 15 women, all university students, served in this study. Percentile scores on the Thorndike College Entrance Examination were obtained for every *S* from the University files.

Results

The means and standard deviations of the various tests for the 30 *Ss* are as follows:

	Mean	S. D.
Serial reaction	209.7	25.30
Simple reaction	198.0	20.70
Reading rate with comprehension	217.9	70.42
Thorndike percentile scores	74.3	12.14

The score for serial reaction is the number of individual discriminative finger movements made in a 2 min. period. Simple reaction is expressed in thousandths of seconds, while reading rate is given in words read per min.

According to Wood,[5] Thorndike scores have a reliability of r = 0.85. The reliability coefficients of the serial reaction and simple reaction measures are $r_{1I} = 0.96$ and $r_{2II} = 0.93$ respectively.[6] The reliability of the reading test scores is related in part to the number of words deducted from the gross score of each observer for an incorrect answer to a comprehension question. The following table presents this observation.

Penalty					
0	words	per	question,	r_{3III}	=0.88
5	"	"	"	"	=0.90
10	"	"	"	"	=0.94
20	"	"	"	"	=0.95
30	"	"	"	"	=0.94
40	"	"	"	"	=0.83

It appears from the above array that some *Ss* attempted to increase their speed of reading beyond their comprehension rate. This resulted in errors on the comprehension questions as well as unstable measures of reading rate. By the use of weights on the comprehension questions, the relative extent of this instability is revealed. For the purpose of correlation with other measures, the reading scores with a reliability of $r_{3III} = 0.95$ are used.

Correlations among the four measures are as follows:

Serial reaction and simple reaction	$r_{12} = -0.09$
Serial reaction and reading rate	$r_{13} = 0.23$
Serial reaction and Thorndike scores	$r_{14} = 0.25$
Simple reaction and reading rate	$r_{23} = 0.13$
Simple reaction and Thorndike scores	$r_{24} = 0.02$
Reading rate and Thorndike scores	$r_{34} = 0.32$

[5] B. D. Wood, *Measurements in Higher Education*, 1925, 45.

[6] Each reliability coefficient was obtained by correlating the first half of the particular test with the second half. The coefficient then was corrected for double length of the test by the Brown-Spearman prophecy formula:

$$r_{11} = 2r_{\frac{1}{2}\frac{1}{2}}/(1 + r_{\frac{1}{2}\frac{1}{2}})$$

First and second order partial correlations closely parallel the zero order coefficients; hence, there are no borrowed factors in the coefficients given above. Neither are there interference factors which tend to obscure relationships greater than those obtained.

By the use of the following formula:[7]

$$\sigma^2\triangle/\sigma^2\triangle \infty \omega = (1/r_{1I} + 1/r_{2II} - 2)/(1/r_{1I} + 1/r_{2II} - 2r \infty \omega),$$

a quotient is procured, which, when translated from a table, gives the percentage of measurable differences in test scores which exists in excess of chance differences. By the application of this technique to the above inter-correlations, the following measurable differences readily are determined.

Serial reaction and simple reaction	88.8%
Serial reaction and reading rate	86.9%
Serial reaction and Thorndike scores	75.0%
Simple reaction and reading rate	85.1%
Simple reaction and Thorndike scores	78.2%
Reading rate and Thorndike scores	72.0%

The high percentage of measurable differences between serial and simple reaction in adults indicates that the speed of reaction of a muscle group in the forearm, wrist, and fingers, which is set for a specific response, does not determine the speed of reaction of a series of similar muscle groups which respond successively with no opportunity for a preparatory set.

Further, the measurable differences among simple and serial reactions and reading rate are approximately the same as the differences between the two motor tests themselves. In addition, it should be observed that the correlation between serial reaction and reading rate is higher than that between simple reaction and reading rate; yet, there is a higher percentage of measurable differences in the former. This fact prohibits direct interpretation from correlation coefficients alone.

Finally, the percentage of measurable differences between the Thorndike test and the other three tests is somewhat lower because the reliability of the Thorndike is 0.85. Nevertheless, the percentage of differences suggests that speed in simple reaction, serial reaction, and reading plays a minor rôle in the determination of intelligence as it is measured by the Thorndike Examination.

Conclusions

(1) In adults, there is little relationship among measures of speed of simple reaction, serial reaction, and reading rate. The percentages of measurable differences in excess of chance differences are 88.8%, 86.9%, and 85.1%.

(2) Simple reaction, serial reaction, and reading rate are not the principal determinants of proficiency on the Thorndike Entrance Examination. The percentages of measurable differences are 78.2%, 75%, and 72%, respectively.

[7] T. L. Kelley, Professor Kelley's reply, *J. Educ. Psychol.*, 16, 1925, 52-55.

A COMPARISON OF METHODS IN DETERMINING THE AFFECTIVE VALUE OF WORDS

By Richard Ledgerwood, University of Maine

The object of the present study was to investigate the application of two of the traditional psychophysical methods, the method of paired comparisons and the order-of-merit method, to the aesthetic judgment of verbal material.

Procedure. It was desired to obtain, for use as stimulus-material, plausible combinations of letters which should be free of denotative meaning. The artificial word-formations which figure in great numbers as the names of characters and places in the novels of James Branch Cabell supplied this requirement. A list of the proper nouns occurring in one of these novels, *The Silver Stallion*, was compiled. This list was first subjected to certain processes of elimination. Formations bearing a discernible likeness to known words were excluded from consideration, as were also compound names and formations possessing diacritical marks. From the remainder, 20 combinations of letters were selected with an eye to variety of constituent letters and diversity in length. The selection was made in such a manner that groups judged by simple inspection of the compiler to be extremely pleasing in character, mediocre, and distinctly ugly contributed about equal numbers.

Arranged in alphabetical order, the words chosen were:

Apsarasas	Fomor	Miramon	Storisende
Breidablik	Glavrab	Ninzian	Thossakan
Coth	Holmendis	Ogde	Vanadis
Donander	Kerin	Porutsa	Yaotl
Ettarre	Ladinas	Ranec	Zipacna

Each of these 20 words was paired twice with each of the other words, positions being reversed the second time so that every word occurred in combination with every other word once on the right and once on the left. The 380 pairs so obtained were arranged in chance order and typewritten on 16 slips of paper measuring 11 by 4¼ in. Twenty-five pairs appeared on each of the first 15 slips and the remaining five pairs on the last.

The 7 members of a class in experimental psychology, comprising 4 men and 3 women, acted as judges. They first made the judgments by paired comparison, underlining the word of each pair that they preferred to the other. When they had completed this task, they were given a simple list of the 20 words and asked to arrange them in order of choice according to the order-of-merit method.

Results. The sum of the frequency of choices on the method of paired comparisons was found for each word. The sum of the ranks for each word on the order-of-merit method was also determined. The order of size of these figures

*Accepted for publication May 21, 1931.

is shown in Table I. The order of the composite judgments as determined by the method of paired comparisons is shown in the first column; and the order of the composite judgments as determined by the order-of-merit method is shown in the second column.

TABLE I

ORDER OF COMPOSITE JUDGMENTS

Words	Methods		Words	Methods		Words	Methods	
	P.C.	O.M.		P.C.	O.M.		P.C.	O.M.
Vanadis	1	1	Storisende	8	8	Coth	14	17
Donander	2	2	Thossakan	9	10	Ranec	15	11
Miramon	3	3	Breidablik	10	12	Glavrab	16	16
Ninzian	4	6	Apsarasas	11	14	Zipacna	17	15
Ettarre	5	4	Kerin	12	9	Porutsa	18	18
Holmendis	6	5	Fomor	13	13	Yaotl	19	20
Ladinas	7	7				Ogde	20	19

The results of the two methods concur in showing that *Vanadis*, *Donander*, *Miramon*, *Ninzian*, and *Ettarre* were the 5 names found most pleasing by the judges and *Glavrab*, *Zipacna*, *Porutsa*, *Yaotl*, and *Ogde* the 5 names found least pleasing.

The correspondence of the results yielded by the two methods, measured in terms of the rank-difference coefficient of correlation, was ascertained to be + 0.96. This figure agrees very well with Barrett's finding of an average correlation of 0.987 for three non-aesthetic materials—weights, handwriting, and beliefs.[1]

The amount of agreement among the judges was determined by computing the rank-difference correlation of each *S*'s judgments with each other *S*'s judgments, treating the results of the two methods separately. This procedure gave 21 coefficients to be averaged for the method of paired comparisons and a like number to be averaged for the order-of-merit method. The average agreement between judges was + 0.48 on the method of paired comparisons and + 0.44 on the order-of-merit method. The average agreement between judges on the particular material used when the results of both methods are combined was + 0.46.

[1]Mabel Barrett, A comparison of the order of merit method and the method of paired comparisons, *Psychol. Rev.*, 21, 1914, 278-294.

APPARATUS

AN ACCURATE DIFFERENTIAL COLOR MIXER

By Forrest L. Dimmick, Hobart College

The differential color mixer on which the proportions of two or more color disks can be changed while the machine is in operation has long been a favorite device for color demonstrations. There are several forms of the instrument on the market which carry from 2–6 independently variable disks. In the main, however, the shifting mechanisms introduce considerable errors so that only the approximate amounts of exposure of the color disks at any moment can be read from the scale. A machine of this type, which we have built in the Hobart Laboratory, was designed with precision in view. Only two disks are carried by this rotator. Each is fastened to the end of one or the other of two hollow steel shafts about 12 in. in length which fit one inside the other. The outer shaft is mounted at the ends in ball bearings and is driven by a belt from a 1/6 h.p. motor. Two parallel slots, 1/8 in. wide, exactly 9 in. long and directly opposite each other, run through the middle length of the larger shaft. In the smaller shaft is a similar pair of slots cut in a spiral that makes exactly one revolution in 9 in. A 1/8 in. hardened steel pin passes through the four slots and is carried in the end of a third steel tube that runs inside the smaller shaft. As this third tube is pushed in or pulled out, thus changing the longitudinal position of the steel pin, the relative positions of the two shafts and their corresponding disks are changed. The steel pin drives all three shafts together. The rear end of the tube that carries the pin is held in a thrust bearing. A 1/2 in. steel rod, threaded 18 turns to the inch, passes through a nut in the thrust bearing mounting and into the pin tube. The other end is held against end motion in a bearing in the rear support and is furnished with a crank.

We followed Zimmerman's method of attaching the color disks to the shafts. On the front end of the larger shaft is mounted an aluminum plate 14 cm. in diam. and 1/2 in. thick. Near the periphery is a clamp 3 cm. long for attaching a color disk. The center of the plate is recessed to a depth of 9/32 in. for a diameter of 1/4 in. thick which is attached to the end of the smaller shaft. The smaller plate is fitted with a clamp like that on the larger and an arbor similar to the ordinary one used for holding color disks, save that it does not screw down tightly upon the color disks but holds their centers in place, at the same time permitting them to move freely. The advantages of this instrument lie in the double slot and in the dimensions of the screw. The close fitting pin acting in two places reduces lost motion between the two shafts to a practical zero. Any setting can be duplicated with an error of less than 0.5°. The ratio of the screw (20) to the length of the slots (9 in.) gives 180 turns to a complete 360° shift of the disks or one turn to every 2°. Since the handle of the screw can easily be set by quarter turns, steps of 0.5° can be made. Adjustments of this size have been made throughout the

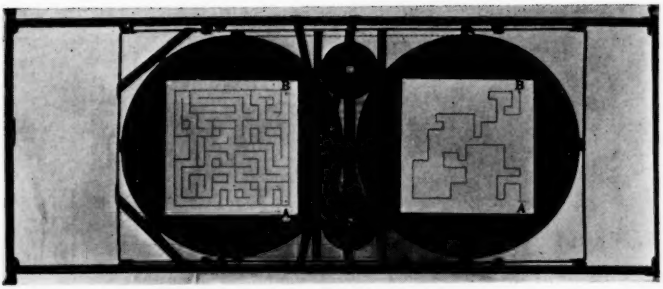

FIG. 1. SELF-RECORDING HUMAN MAZE

entire range and have been found to be as accurate as could be measured with a protractor. The necessity for making 180 turns of the screw to go from all of one disk to all of the other can be obviated by using a split nut in the thrust bearing and providing proper means for opening and closing it according to whether or not the screw adjustment is to be used.

A SELF-RECORDING HUMAN MAZE

By Leon B. Slater, Ohio Wesleyan University

The self-recording human maze described here is an outgrowth of a peg maze devised by J. F. Shepard at the University of Michigan. The added features which make possible the automatic recording of objective behavior in the maze learning process are set forth in the accompanying line cut. The apparatus is mounted on a laboratory table. At each end of the table is a transverse track made of angle-iron on which the whole assembly moves on the larger carriage from side to side. The smaller carriage moves lengthwise on the larger one. In this way means are provided for rotating the maze regardless of the eccentricity of the point selected in the maze as a center for making a turn.

A stationary screen constructed of ordinary wall-board prevents *S* from seeing the plan of the maze but provides him room to reach forward in order to move the maze as he finds it necessary for operating the apparatus. *S* acquaints himself with the maze by looking through a fixed periscope which has a visual field with a diameter slightly more than the width of a single alley.

To run the maze *S* starts with 'A' in the periscopic field. He draws the apparatus toward him until he reaches a turn in the alley. At this time *S* steps on a foot-treadle which forces a sharpened metal peg through a mounting in the table-top and into the bottom surface of the maze disk, thereby holding the visual field at the point of entrance to the next alley as the turn is being made. The *S*s report that by restricting all movement through the alleys to movement toward them, they derive a persistent impression of actually walking or progressing up the pathways until they arrive at the end, 'B.'

A pencil or marking pen which is set up on the second large disk marks out the exact pathways which are resorted to in the series of attempts to learn the true path.

The apparatus has the following advantages over the other human mazes. (1) It records objectively and permanently all errors by registering the direction of the path taken at any point during a run, the extent of all deviations from the true path, the persistence of errors in a series of runs, the elimination of errors as learning takes place, the exact distance covered in each run. (2) It can be used for any type of maze pattern which can be drawn on coördinate graph paper. (3) It provides almost immediately a complete disorientation of *S* in the maze. (4) It can be used by students as a standard piece of laboratory apparatus. (5) It affords more precision in an instrument for research.

APPARATUS NOTES

A Simplification of the Ruckmick-Patterson Non-Polarizable Electrode

In the course of setting up apparatus for the galvanic reflex, we decided to utilize the electrodes described by Ruckmick and Patterson.[1] Since we were inexpert at glass blowing we appealed to the Department of Chemistry for assistance. While waiting we fashioned some substitutes in our own shop that have proven equal in every respect to the more complicated forms of Ruckmick and Patterson.

We took two thistle tubes with long stems (30 cm.) and bent the stem of each tube at about the middle into a 'U' so that the end of the stem was brought up close to the top of the funnel. The 'U' was filled with mercury to the bottom of the bowl. In all other respects, we followed Ruckmick's directions. The advantages that we have to urge for our modification are: (1) the ease of construction, (2) cheapness, and (3) the smaller change in level of the liquid, when the finger is inserted, due to the size and shape of the thistle tube.

Hobart College

Forrest L. Dimmick
Alger L. Adams

A Modification of the Dallenbach Heat Grill

The copper tubes used in the construction of the Dallenbach heat grill[2] are small (1/16 in. bore) so as to bring the two systems of the grill in close juxtaposition and thus to stimulate differentially narrow and closely integrated areas of the skin. This construction meets the requirements of stimulation but it does not yield a flexible means of varying the stimulus-temperatures.

The temperature of each grill-system, it will be recalled, is controlled by forcing water, by means of pressure from the mains, through copper coils immersed in constant temperature water-baths (hot or cold) and by varying the water's rate of flow through each system. Because of the small bore of the grill-tubes, the limits to which the stimulus-temperatures may be changed by this means is small. If these limits are to be exceeded, the temperature of the water-baths must be changed. This alteration is not only time-consuming, but it requires delicate adjustment of special thermostatic apparatus. In order to avoid these complications the grill described here was constructed of ¼ in. tubing which was rolled flat so as to give a bore 1/6 x ½ in. The construction was otherwise similar to the older form.

The larger bore of the grill-systems of the modified apparatus allows a greater volume of water to pass through the tubes and hence increases greatly the limits to which the stimulus-temperatures may be varied. Indeed, it is possible with this modification to obtain the range of temperatures desired merely by varying the rate of flow through baths of boiling and salted ice water. Since the temperature of these baths is constant, thermostatic controls are not required.

Columbia University

Meyer Koch

[1] C. A. Ruckmick and E. Patterson, A simple non-polarizing electrode, this Journal, 41, 1929, 120.

[2] For a description see N. C. Burnett and K. M. Dallenbach, The experience of heat, this Journal, 38, 1927, 419.

NOTES AND DISCUSSIONS

Vocational Counseling: The Interest Inventory

Groups of men who are engaged in certain occupations sometimes have characteristic likes and dislikes which distinguish them from groups following other occupations. Although the careful student of vocational and occupational interests is loath to overgeneralize this fact, immature persons who are trying to decide upon their life work are likely to gain the impression that this finding is of great significance. Students occasionally believe that they may employ a vocational interest inventory to determine, with a fair degree of accuracy, whether they should or should not enter a particular occupation. And the adviser often places unwarranted emphasis upon the alleged interest of the student. This practice has definite limitations which will be discussed briefly in this paper.

Searching for "psychological types." It is interesting that long after a psychological concept has been discarded, it often survives in disguised form. For example, in spite of the recognition of the general applicability of the normal distribution surface, some investigators continue to speak and to write in a manner which shows that they are still thinking in terms of bi-modal or multi-modal distributions. Certain psychologists, dealing with vocational and occupational interests, apparently believe that they have discovered several types of workers; *e.g.* those interested in people, those interested in things, and those interested in ideals. Such a threefold classification is obviously arbitrary; it is also invalid, if it is applied generally, for most individuals are interested in people as well as in things, and also in ideals!

To the present writers, unguarded optimism with reference to the practical application of the vocational interest inventory is comparable to the early attempt to adapt instruction to the needs of "eye-learners" and "ear-learners." Group differences are magnified; similarities are minimized and often overlooked entirely. By selecting those likes and dislikes which differentiate groups in several occupations (and neglecting the large number of likes and dislikes which members of the various occupational groups have in common), the illusion of "types" is created. It may well be that at some time in the future this search for types will prove profitable (scientifically) and that the type theory will yet receive verification in the hands of competent investigators. Nevertheless, until the evidence is at hand, the fact cannot be too strongly emphasized that the burden of proof rests upon those who continue to believe in the existence of psychological types.

Adult interests vs. youth's potential interests. The scores on the vocational interest inventory show how nearly a person's interests coincide with those of the average mature individual successfully engaged in particular occupations. The value of this information to the vocational counselor rests upon the validity of the assumption that the individual's interests, preferences, likes and dislikes, are somewhat constant factors which may be measured and labelled before the individual has reached maturity, and also before he has had occupational experience. This assumption disregards the fact that motivation modifies and indeed creates interests. According to Woodworth, "almost any object, almost any act, and par-

ticularly almost any process or change in objects that can direct one's own activity towards some definite end, is interesting of its own account, and furnishes its own drive, once it is fairly initiated."[1]

Assume that mature, experienced workers in one group *do* have preferences and inclinations which differentiate them sharply from those in other occupational groups. Does it follow that as youths these workers possessed their present stock of interests? Hardly! The occupational *knowledges* possessed by various kinds of workers probably differentiate occupational groups—even more sharply than do the characteristic likes and dislikes. It is hazardous to use tests of *occupational knowledge* to identify potentially successful workers, for the knowledges are to a large extent the result of acquisition. Woodworth's assertion (as well as common sense) suggests that most *interests* are acquired. Surely, successful endeavor is one fact of great importance in creating occupational interest. It is extremely unlikely that the chaotic and frequently undirected activities of growing youth will lead to interests that have much bearing upon occupational fitness or success.

It will be of interest to trace the genesis of one occupational interest. A salesman tries to sell certain products. He calls on his prospective customers week after week. In the course of time he becomes accustomed to meeting and dealing with people. He becomes 'interested in people,' *e.g.* his prospective customers. Does this suggest that his interest in people is due largely to the fact that he is, by original nature, or as a result of early experience, "interested in people"? His interest is likely to be ulterior, and often it is clearly acquired. Of course, one may become habituated to associating with people, and therefore one may acquire a genuine 'interest' in people. It is more probable that the interest in people is not a *general* interest or a true reflection of innate sociability. The sociability is likely to be the reflection of a habituated response from necessary contact with certain people. It is therefore misleading to point to certain prospective workers catalogued as those *interested in people*. It is extremely likely that many, if not most occupational interests, are acquired as correlates to particular occupational endeavor. Prediction of these adult interests would be therefore unlikely from a study of the reactions of these persons in youth.

The origin and the derivation of 'interests.' In connection with the preceding analysis of the development of interests, the following question is pertinent. What is it that initiates and directs vocational interests? A partial answer is given by Douglas Fryer, who studied the interest histories of forty superior adults, and listed five general causes of the development of interests.

"First, perhaps, is the economic cause, which is seen in almost every history. Vocations are determined, which are very happy choices, by the fact that they minister to the vital needs of life. A second cause, possibly equally important, is the chance stimulation of the environment. Interests are made by the fact that the individual has chance contact with certain objects and activities. A third cause, also equally important, and the one most often mentioned in these histories, is a personality, or personalities, older than the subject, who suggest a line of development.... The personality influence as a determiner of interests has, it would seem, been neglected in the understanding of the development of interests. Fourth, as a causative factor, is the general motivation of the individual to be recognized among his fellows. This is an obvious cause to most clinicians. The intrinsic factors of an interest seem less important in most of these histories, but they constitute a

[1]R. S. Woodworth, *Dynamic Psychology*, 1918, 202.

fifth general cause in the development of interests. . . . it will usually be observed that the prestige of a vocation, the general motivation to be somebody in relation to one's fellows, is equally important. . . . Generally, intrinsic interests seem not to be strong causative factors in the development of the interests of these subjects."[2]

Fryer states that economic need and the desire for social recognition are extremely important determiners of vocational interests. Chance stimulation of the environment and the example of influential friends also are actuators of interest. Possibly these causes of 'interests' explain in part why scores on the interest inventories ordinarily fail to show any marked correlation with measures of occupational success. In many instances, particularly among young workers, the interests develop with experience, chance relationships, and countless other environmental factors. These factors are difficult to anticipate, and therefore they are seldom represented in the interest inventory.

'Interest' scores as predictors of future success. Certain early investigators believed that interests might prove indicative of ability and therefore might be valuable prognisticators of occupational success. Nevertheless, vocational interest scores show practically no correlation with degrees of vocational success. Ream administered the Carnegie Interest Inventory to groups of successful and unsuccessful salesmen.[3] The scores were ascertained for the successful and for the unsuccessful, and differential score patterns were established. The experiment was repeated with a new group of salesmen, but the differentiation could not be similarly established from the scoring pattern.

Craig made a comparable effort to differentiate between successful and unsuccessful saleswomen.[4] Excellent differentiation was obtained within the primary groups; a scoring key was then devised. When a control group was studied similarly, the scoring key failed to differentiate the successful from the unsuccessful. Freyd tried to distinguish between successful and unsuccessful salesmen,[5] and he obtained results similar to those of Ream and Craig. Kornhauser attempted to distinguish successful from unsuccessful college students by studying their interests.[6] His attempt also was unsuccessful.

For the occupations that he has thus far studied, Strong reports a negligible coefficient of correlation between degree of vocational success and vocational interest score.[7] Garretson,[8] who worked with ninth-grade boys, reports that "little relationship exists between the degree of preference for and ability in a curriculum as measured by objective tests of curricular aptitude. The range of coefficients of correlation between preference scores and measures of curriculum ability is $+0.190 \pm .055$ to $-0.304 \pm .52$."[8]

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[2] D. Fryer, *The Measurement of Interests*, 1931, 413-414.

[3] M. J. Ream, *Ability to Sell: Its Relation to Certain Aspects of Personality and Experience*, 1924, 47-48.

[4] R. D. Craig, The preference-interest questionnaire in selecting retail saleswomen, *J. Personnel Research*, 3, 1924-1925, 366-374.

[5] M. Freyd, Selection of promotion salesmen, *J. Personnel Research*, 5, 1926-1927, 142-156.

[6] A. W. Kornhauser, Results from a questionnaire of likes and dislikes used with a group of college freshmen, *J. Appl. Psychol.*, 11, 1927, 85-94.

[7] E. K. Strong, Jr., *Manual for Vocational Interest Blank*, 1930, 6 f.

[8] O. K. Garretson, Relationships between expressed preferences and curricular abilities of ninth grade boys, *Teachers Coll. Cont. Educ.*, 396, 1930, 72.

These data show conclusively our inability to distinguish between successful and unsuccessful individuals by means of the present-day interest inventories.

Of course this situation may not obtain for all kinds of work and workers. Group interests may some day be identified which will differentiate successful from unsuccessful workers within a particular occupational field. Nevertheless, it seems to the present writers that the burden of proof rests upon those who assume that interests are highly indicative of ability.

The interest inventory and the high school pupil. In view of the foregoing data, the writers believe that it is unwise to give boys of high school age the impression that they may follow their present inclinations to ultimate advantage. In some instances such a policy might be justified by the results. In a far larger number of instances it probably would not prove fruitful. In the growing individual, interests and inclinations are not constant factors; they are subject to change. Abilities not yet tapped may be the potential sources of new interests and inclinations. Moreover, intensity of effort during the initial periods of almost any endeavor is likely to be accompanied by dissatisfyingness. Later the activity itself may furnish the stimulus to further activity, but the initial periods are frequently attended by dissatisfyingness. This is particularly true in the initial periods of developing a skill, but it is probably true also in the case of the young worker when he is becoming adjusted to his life work. There are also many vocational interests which are transitory. Indeed, it would be difficult to find a characteristic of a growing boy more subject to change than his vocational ambitions.

Many recognize the fact that interests often are transitory, and that our present stock of guidance devices are inchoate. For example, Strong states that he has obtained no data for high school boys,[9] and that it will be many years before the validity of employing his inventory for counseling high school youths can be determined.[10]

Strong therefore urges caution in the use of his inventory. Nevertheless, the interest inventory is now being used occasionally by optimistic and over-enthusiastic counselors. In his recent book, *How to Find the Right Vocation*, Kitson describes how the device may be employed.

"The test [the interest inventory] has been given to workers in about thirty occupations and count has been made of the frequency with which the workers in a single occupation, such as accountancy, prefer certain things in the list. Then it is given to a boy just being graduated from high school or just entering college. His likes and dislikes are then compared with those of the persons in these thirty occupational groups. If he likes the same things as those liked by the accountants, he is encouraged to enter that occupation. If, however, his likes correspond to those of physicians, he is encouraged to enter medicine." [11]

Although Kitson mentions some of the limitations of the interest inventory, he does not present the limitations in such a way that proper caution will be exercised by those who are trying "to find the right vocation."

The Vocational Interest Quotient (V.I.Q.). There has recently come to the attention of the writers a device for ascertaining pupils' Vocational Interest Quotients (V.I.Q.). The Manual of Directions that accompanies this device contains the following explanatory statement.

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[9] *Op. cit.*, 6. [10] *Op. cit.*, 1.

[11] H. D. Kitson, *How to Find the Right Vocation*, 1929, 134.

"For a number of years, I have sought to devise a convenient measure of vocational interest that would be *as helpful in its field as the I.Q. has been in mental testing.* My investigations have resulted in the invention of the Vocational Interest Quotient (V.I.Q.) and I feel that these have now been carried far enough to warrant *practical use* of the device and to invite wider coöperation in *further establishing its usefulness*" (italics ours).[12]

In the judgment of the present writers the foregoing attempt is at present unwarranted as well as injudicious. It is rather commonly charged that the psychologist often presents new concepts or laws before he has verified them. The use of the V.I.Q. seems to be an instance of presenting a new method (which will have significant social consequences if it is used) before it has been thoroughly tested. Why should the V.I.Q. be put to "practical use" *before* its usefulness has been thoroughly established? Certainly, the present writers have been unable to find evidence which shows any significant relationship between V.I.Q. scores and anything else of significance in demonstrating the validity of the device.

Other aspects of the V.I.Q. merit attention: the similarity of the names, I.Q. and V.I.Q., and the attempt of the author to imply similarity between the two measures. The symbol I.Q. is of course widely known and its significance is generally understood. The novice may assume that the I.Q. and V.I.Q. are alike in many respects. Perusal of the V.I.Q. literature reveals that this supposition has no basis in fact. For example, the I.Q. represents ability that is largely native and relatively constant or stable. The I.Q. therefore makes certain kinds of prediction possible. The V.I.Q. probably does not reflect native as contrasted with acquired abilities; the constancy of the V.I.Q. pattern certainly has not been demonstrated; the individual has not one but several.

The writers do not intentionally belittle sincere and honest research in any field whatsoever. What the writers do object to is the "*practical use*" of a tool, the validity of which has not been demonstrated scientifically. The Manual of Directions instructs the examinee to compute his V.I.Q. for the occupations in the booklet.[13] He should then "rank his V.I.Q.'s in descending order and consider most seriously those occupations that are in the upper quarter of rank."

Even the novice in psychology must realize that the suggestibility of pupils would nullify much of the validity of data obtained by such a procedure. As a research tool the vocational interest inventory has a legitimate place; the present writers submit that, in our present state of knowledge, its "practical use" is a bit premature.

Ohio University — Harvey C. Lehman
Northwestern University — Paul A. Witty

The Intensive Factor in the Localization of Sound

The purpose of this note is to suggest a possible means by which dichotic intensity-differences mediate localization of sound by virtue of their reduction to time-differences. If we assume the essential identity of phase-differences and time-

[12] H. W. Hepner, *Skilled Trades for Men: Directions for Finding Your Vocational Interest Quotients (V.I.Q.)*, 1931, 1.

[13] Hepner, *Manual of Directions for Counselors Using Vocational Interest Quotient Booklets*, 1931, 4.

differences, as stated by Boring,[1] all localization of sound might then be supposed to depend on a single, peripherally effected criterion.

If the fibers of the basilar membrane vibrate in resonance to a given imposed frequency, we may follow Banister's[2] suggestion that nerve-impulses in the auditory fibers are initiated when, as far as the cilia are concerned, a certain phase of their cycle has been reached. Perhaps it is more accurate to say, with Troland,[3] that the liberation of the nerve-impulses takes place at a more or less definite magnitude of displacement of the basilar membrane. The movement of the basilar membrane would then be correlated with a variable amount of tension or pressure on the hair cells. Hartridge[4] thinks that impulses are initiated during the downward movement of the basilar membrane, a belief which implies that the threshold of stimulation corresponds to a more or less fixed amount of tension on the hair cells. With very weak intensities the displacement of the basilar membrane would be insufficient for the requisite tension. With great intensities, however, the basilar membrane would continue to move after the threshold tension had been attained. When a sound is just audible, we may suppose that the threshold tension is reached at the point of maximum displacement of the basilar membrane, whereas in the case of intense sounds the threshold must be reached before the peak of the cycle.

The process outlined thus far is supported by Troland's[5] conception of the initiation of the series of temporal differences responsible for the curvature in the neural wave-front, which he presents as the neurological correlate of the experience of tonal brightness. The center of the displaced strip of basilar membrane moves most rapidly and stimulates hair cells soonest. From the center towards either end of the displaced area, movement of the basilar membrane is progressively slower, and impulses are initiated progressively later in the cycle. It seems that this notion is capable of a further extension. When frequency is diotic and intensity dichotic, a certain fiber or group of fibers in the more intensely stimulated ear must initiate impulses prior to any of those initiated in the less intensely stimulated ear. In this way dichotic intensity-differences are correlated with differences in time.

This conception may be invoked to explain the superiority of dichotic phase-differences over dichotic intensity-differences in auditory localization. In the case of intensity-differences there must be a series of time-differences corresponding to the wave-fronts in both ears. In other words, the differences of time include more than those of the hair cells responding to maximally displaced basilar fibers in both ears. The spacing of the temporal factor might well reduce accuracy of localization.

If, as Hartridge thinks, the hair cells are stimulated only during the downward movement of the basilar membrane, it follows that the upward movement is lost

[1] E. G. Boring, Auditory theory with special reference to intensity, volume, and localization, this JOURNAL, 37, 1926, 164-173.

[2] H. Banister, A suggestion towards a new hypothesis regarding localization of sound, *Brit. J. Psychol.*, 17, 1926, 142-153.

[3] L. T. Troland, The psychophysiology of auditory qualities and attributes, *J. Gen. Psychol.*, 2, 1929, 28-56.

[4] H. Hartridge, A vindication of the resonance hypothesis of audition, *Brit. J. Psychol.*, 12, 1922, 362-382.

[5] Troland, *op. cit.*, 47-51.

in so far as excitatory effectiveness is concerned. The limit of time-variation depends on the amount of displacement necessary for excitation. Assuming that the limiting points are the position of rest and the position of greatest downward movement of the basilar membrane, ¼-wave would be available. But this is a minimum, since the hair cells in their 'resting' positions might be past the points of discharge. Adaptation would prevent response of the hair cells in their 'resting' positions, but the level of adaptation might well change, so that with continuous stimulation the limiting positions could conceivably become the maximum upward and the maximum downward displacements of the basilar membrane. In this way, ½-wave might become the outside limit of time-variation.

It must be admitted that the establishment of a temporal gradient as the essential factor in auditory localization would complicate the conception of brain events. Boring[6] has argued for the relation of conscious dimensions to the physical dimensions of brain processes. An intensive shift in the brain may be correlated with a spatial shift in the excitatory mode, and seems natural as the ground for a change in localization. With time-differences as the primary criteria, it becomes necessary to translate them somehow into spatial dimensions. Nevertheless the establishment of concomitant time-differences with differences of intensity does not create any essentially new theoretical difficulties, since any assumptions concerning cortical events with dichotic phase-differences and time-differences may be extended to account for the time-factor derived from the intensity-factor as suggested in this paper.

Tufts College W. A. Bousfield

The Progressive-Weights Test of Suggestibility with Adults

A lifted-weight test devised by Binet for measuring suggestibility in children[1] was adapted by the writer and given to 187 *Ss* in the Ozark highlands. The *Ss* were drawn from a region divided occupationally between agriculture, commerce, manufacturing, and mining, coming from every size of community, from open country to a city of 35,000. The age-range was 17-55 yr., with the mean at 30. There were 90 men and 97 women. Social and economic backgrounds varied widely, occupations ranging from unskilled labor to professional service. Ten (5.5%) had less than an eighth-grade education; 42 (22.5%) had completed the eighth grade; 84 (44.8%) had had 1-4 yr. of high school; 6 (3.2%) had technical or professional training. The *Ss* therefore form a representative sampling of the local population, within the age range set.

Before submitting to the test, all had marked a ballot on matters of belief and opinion in religion,[2] and had worked with a line-length test.[3] None were familiar with the psychological laboratory and its methods, a high percentage were suspicious of anything resembling a clinic or operating room, and in preliminary experimentation many showed that they suspected magic or trickery. The 'weights'

[6]Boring, The physiology of consciousness, *Science*, 75, 1932, 32-39.

[1]A. Binet, La suggestibilité, 1900, ch. iv. Cf. G. M. Whipple, *Manual of Mental and Physical Tests*, 1921 (3rd ed.), vol. 2, 232-236.

[2]See P. R. Stevick, A sampling of Ozark creeds, *Mountain Life and Work*, 7, 1932, 20-23.

[3]Whipple, *op. cit.*, 242-246.

test was therefore given in the home or place of employment, on any table or counter or work-bench of the right height (30 in.) available.

A set of 15 weights, identical in size and appearance, with the exception of conspicuous numbering 1-15, were used. The first four weighed 20, 40, 60, and 80 grm., respectively, the remaining eleven weighed 100 grm. each. They were arranged in serial order, with No. 1 at the left, and about 2 cm. between weights. While they were being arranged, *E* took care to exhibit the brass end, showing that the shell was a dummy. He also called attention to the identical size, shape, and color of the weights and to the number at the top.

The instructions to *S* were as follows:

> What I want you to do is to lift each one just once, like this (here *E* began lifting weights one at a time), and as you lift it you are to tell me whether it is heavier, lighter, or the same as the one just before it (by this time *E* had lifted the first four weights). Do you see? You lift each weight *just once* (here *E* demonstrated a second time) and as you lift it you tell me whether it is heavier, lighter, or the same as the one just before it.

As each weight was lifted, *S*'s report was recorded. If he hesitated he was asked, "What is your first guess?"

The number of 'heavier' judgments after Weight 5 until a 'same' or 'lighter' judgment broke the progressive series constitutes the score. Only 3 *S*s, a chemical engineer, a dairyman, and a library desk clerk, judged the entire series correctly. The frequencies for scores 0-10 are 116, 35, 11, 8, 3, 3, 2, 1, 1, 1, 6. Sixty-two percentage (116) show no signs of a suggestion of progressive increase. Only about 20% register more than 1 score-point in suggestibility. Failure of the test to yield anything approximating a normal distribution indicates its extremely limited value with untrained adults. The *r*, moreover, for correlation with a measure of feeling of conformity to majority opinion on religious belief is −0.007. This is in line with recent research, which points away from such general traits as suggestibility.

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THE METHOD OF SINGLE STIMULI

An increasing interest in absolute judgments and absolute methods makes it desirable to revise the terminology used in discussing these judgments and methods.[1] The following terms have emerged from a consultation with Professors E. G. Boring, S. W. Fernberger, and C. C. Pratt.

(1) *Upper category:* those reports which will surpass the other reports in frequency if the stimulus-magnitude is sufficiently increased.

(2) *Lower category:* those reports which will surpass the other reports in frequency if the stimulus-magnitude is sufficiently decreased.

(3) *Intermediate category or categories:* those reports which will decline in frequency as the upper category increases on one side, and as the lower category increases on the other.

[1]Two principal references are: E. G. Wever and K. E. Zener, The method of absolute judgments in psychophysics, *Psychol. Rev.*, 35, 1928, 466-493; S. W. Fernberger, On absolute and relative judgments in lifted weight experiments, this JOURNAL, 43, 1931, 560-578.

(4) *Limen*, with the symbol L: that value of stimulus, arrived at by interpolation, at which the probability of any given report except an intermediate report equals the summed probabilities of the other reports.

(5) *Upper limen*, with the symbol L_U: the limen of the upper category.

(6) *Lower limen*, with the symbol L_L: the limen of the lower category.

(7) *Central interval*, with the symbol CI: $L_U - L_L$ taken without regard to sign.

(8) *Point of symmetry*, with the symbol PS. Experimenters must choose between the following definitions:

(a) the mean of the upper and lower limens, $(L_U + L_L)/2$;

(b) Xi, the point at which the psychometric functions of the upper category and the lower category intersect;

(c) the mean of the distribution of reports in the intermediate category;

(d) the mode of the distribution of reports in the intermediate category.

(9) *Method of single stimuli:* any psychophysical method in which a report follows the presentation of one stimulus only.

The first six definitions apply to the method of constant stimuli or to the method of constant stimulus differences.[2] L is Urban's S. *Central interval* corresponds to *interval of uncertainty* in the method of constant stimulus differences, and the definition is the same; it seems best, however, to avoid the term *uncertainty* because it is not clear what that term would mean in an absolute method. *Point of symmetry*[3] takes the place of *point of subjective equality*, because *equality* has no meaning in an absolute method. Formulae for calculating PS are given elsewhere.[4]

A psychophysical method may be designated either by the process of judgment which the observer is believed to use, or by the general arrangement of stimuli appropriate to the method. There may be disagreement about the former, but not about the latter; hence *method of single stimuli*[5] seems better than *absolute method*. *Method of single stimuli* quite properly includes the method of constant stimuli, which is used in determining stimulus-limens and terminal limens. *Absolute judgment* and *absolute method* are misnomers if *absolute* is opposed to relative: an absolute judgment upon one stimulus is not independent of similar judgments upon preceding stimuli, and absolute judgments are accordingly relative to other absolute judgments. But *absolute* has a proper meaning if it is opposed to *comparative*: an absolute judgment need not involve a comparison of the given stimulus with some given preceding stimulus. In this sense, terms such as *absolute judgment, absolute impression, absolute series* are held to be admissable.

Harvard University JOHN VOLKMANN

[2]The substance of several of the definitions is taken from E. G. Boring, Urban's tables and the method of constant stimuli, this JOURNAL, 28, 1917, 280-293.

[3]This term has already appeared: S. W. Fernberger, Time relations for the different categories of judgment in the 'absolute method' in psychophysics, this JOURNAL, 44, 1932, 509.

[4]Boring, *op. cit.*, 288 f.

[5]C. S. Myers has used the term *method of single exposures* to designate an absolute method: *Text-book of Experimental Psychology*, I, 1911, 306 f.

The Seventh Annual Meeting of the Midwestern Psychological Association

On Friday and Saturday, May 13 and 14, the University of Indiana at Bloomington was host to the Midwestern Psychological Association during its seventh annual meeting. The meetings were largely attended and a wide geographical distribution of states was represented. Professor Herbert Woodrow, Head of the Department of Psychology at the University of Illinois, served as President. There were seven sectional meetings during which 36 papers were read and discussed and 15 laboratory reports were given. These papers had been carefully selected by the Program Committee from among three times that number which were offered. Besides these regular sessions there was a symposium on learning, which was held on Friday evening under the chairmanship of President W. L. Bryan of the University, who also served as toastmaster at the annual dinner which was held at the Memorial Union Building on Saturday evening. The Department of Philosophy invited the members to a lecture by Professor E. S. Ames (Chicago) on "Imagery and Meaning in Religious Ideas" held on Thursday evening. The annual business meeting occurred on Saturday afternoon. The principal items of business were the reports from the Council, from the Nominating Committee, and from the secretary. It was decided to accept the invitation of the Iowa State College at Ames for the next meeting to be held preferably during the first week of May, 1933. The Council of the Association attended a dinner on Friday evening after which a short business session was held. The Committee of Fifteen on the first course in psychology was represented by nine members and met on Friday morning to discuss the reports of its sub-committees. Professor J. P. Nafe was elected to the general committee to fill the vacancy caused by the death of Professor A. P. Weiss.

The Psi Chi National Honorary Fraternity held a luncheon on Friday noon. President and Mrs W. L. Bryan invited the members of the Association to tea at their residence at the close of the sectional meetings on Friday afternoon. Motor trips were also organized to provide members with an opportunity of visiting some of the scenic landscapes along Hubbard's Ridge in the State Park of Brown County.

The first paper presented during the symposium on learning was on "Gestalt Psychology and Learning" by Professor R. H. Wheeler (Kansas). It was discussed by Professor J. A. McGeoch (Missouri). It was followed by a paper on "The Double Rôle of the Primary Stimulus in Conditioning" by Professor E. A. Culler (Illinois). The third paper was presented by Professor J. Peterson (Peabody College for Teachers) on "Some Developments in the Psychology of Learning in the Last Thirty Years" to which Professor H. B. English (Ohio State) responded. There was a very lively discussion on the part of the audience, principally of Dr. Wheeler's paper, in which Professor C. E. Spearman (London) participated.

In the first section, over which L. L. Thurstone (Chicago) presided, L. D. Goodfellow (Northwestern) compared the ability of the eye, ear, and finger-tip to discriminate short intervals of time. Through the use of three psychophysical procedures, right and wrong cases, just noticeable differences, and average error, he showed that audition gave the smallest differential threshold, cutaneous pres-

sure the next smallest, and vision the largest. There were considerable individual differences in the absolute thresholds, in the amount of learning, and in the relative threshold values for the different techniques. J. P. Cartzdafner (Ohio Wesleyan) followed with a comparison of the psychophysical methods. By means of the procedure of average error, of limits, and of constant stimuli and through the use of specially devised apparatus, which was a modified form of Galton bar, he obtained 5100 judgments from 10 *Os*. He computed the difference-threshold as the average transitional point between the 'just perceptibly different' and the 'just imperceptibly different' points. He concluded that the judgments of equality in the constant method are so significant that they should not be discarded; that the assumption regarding the limits of the tails in the curve of distribution is valid and verifiable; that the differences in the threshold as revealed by the various psychophysical procedures are a function of the procedures themselves and not a function of the mathematical processes involved; and that within the scope of this experiment the constant procedure gives the lowest threshold values and the greatest precision. R. Leeper (Arkansas) furnished "the evidence for a theory of neurological maintenance of states of emotional motivation." He cited examples to show that the central nervous tissues, in contrast with the peripheral, may follow different principles of operation; that the cortex is capable of independent maintenance of simple attitudes; that the subcortical structures like the thalamus must be capable of persistent independent activity; and that there may be similar analogous mechanisms in the cortex for the maintenance of motivation as a result of training. J. R. Patrick and E. Rowles (Ohio) reported the effect of various stimuli on basal metabolic rate, blood pressure and galvanic reflex. "Adrenalin and emotion" was the subject of a paper offered by C. Landis (N. Y. Psychiat. Inst. & Hosp.). By injecting adrenalin into normal and psychopathological individuals he found a wide variation in mental and physiological results. But in some cases a genuine pleasant or unpleasant emotional experience was aroused. He did not believe the theory of imbalance of the divisions of the autonomic system was substantiated by his results and that the emotional experience must always be related to the entire environmental setting in which intellectual processes may also play an important rôle. The session was closed by a paper read by E. Jacobson (Chicago) on the electrophysiology of mental activities. By means of apparatus developed to measure transient potential differences as small as one millionth of a volt, he has been able to measure neuromuscular states during such mental activities as recollection, and visual, auditory, and kinaesthetic imagination. Specific muscles contracted during each type of mental activity investigated.

Under the chairmanship of J. B. Miner (Kentucky) a parallel section of papers opened with a study by M. H. Young (Indiana) on clinical psychology in industry and the professions. The author is continuing the work begun by her late husband, Dr. H. H. Young, who laid out an adjustment program with a large industrial concern. In conjunction with coöperating medical experts she has analyzed the mental difficulties of 54 subjects and classified their difficulties for further psychological study. R. Stagner (Wisconsin) then presented a paper on the relationship of certain personality traits to academic aptitude and achievement. A number of traits, like those of neurotic temperament, self-sufficiency, and dominance were investigated in connection with academic maladjustments. He indicated that stable individuals, very self-sufficient students, and those receiving

high scores for dominance, achieve academic results that are more closely related to their capacity than are those found in contrasted groups. H. L. Koch (Chicago) next read a paper entitled "A Study in Rating Technique with Special Reference to Activity in Pre-School Children." Fifty pre-school children were observed in 9 types of pursuits. The author suggested the danger of using a child's behavior during one type of occupation as an index of his behavior during all others and she advised the importance of taking interactivity correlations into account. Activeness seemed to be influenced in a complex way by room-temperature but not by humidity within the ranges studied. H. M. Williams (Iowa) reported an analytical study of the expressive use of language by pre-school children. Using 72 children he found that development occurred along three lines: the skill in the mechanism of speech; the organization of the sentence; and extent of vocabulary. The correlation between scores and intelligence were of the order of 0.6. A scale of block constructions for young children was proposed by M. W. Bailey (Ohio Wesleyan). Working with 44 children, ranging from 2–9 yr. of age, a standardized project of building anything they wished was developed as a graded series which was judged in terms of 10 photographs ranked according to the method of equal appearing intervals. F. N. Maxfield (Ohio State) next discussed the picture element in certain test material, and raised the question whether several of the puzzle-picture tests were not really form tests. He made a proposal involving true pictures of the completion type for very young children. The last paper was read by D. Gohring (Wisconsin) on variations in strength of emotional responses in newborn infants. Sixty infants from 1–10 days of age were tested for strength of motor, vocal, and blushing responses measured on a 4-point scale under the situation of restraining the arm-activity for 40 sec. and of removal of support by jerking the blanket. Motor responses to jerking the blanket seemed to be non-emotional righting reflexes in regaining balance; the infrequent vocal responses were usually only a whimper; and blushing responses almost never occurred.

W. F. Book (Indiana) served as chairman of the third section which began the afternoon program. F. McKinney (Missouri) read the first paper on the quantitative and qualitative essential elements of transfer. He was concerned with the problem of ascertaining how many and what elements of a given original situation are adequate to evoke a given response under conditions of transfer. Seventy-five associated four simple geometrical forms with four letters of the alphabet. In an altered situation in which a graded series of modifications or forms were made, the *S* was asked to write the letter associated with each modified figure presented. Transfer was found not to be proportional to the amount of the identical stimulus present but it varied with the position of the modified quantity or with its relationship to the rest of the figure. The relative retention values of stylus and mental maze habits was discussed by R. H. Waters and G. B. Poole (Arkansas). They tested the hypothesis that the apparent better retention of motor habits than verbal habits is due to the factor of greater over-learning. By rigidly controlling the factor of over-learning they concluded that the result was a function of the relative degree of complete learning. A long report of an experiment on learning and time by G. S. Snoddy (Indiana) then followed. On the basis of numerous control situations he came to the conclusion that learning involves adaptation to a whole situation and a total set of temporal conditions. Co-ordination then becomes an item in a total space-time configuration involving a

contraction of these dimensions during the learning period. Learning curves can hence be readily fitted by simple logarithmic equations. Through fractionation of the learning period the impression is gained that all of the elements of the learning curve can be explained in terms of the energy-cycles of thermodynamics. W. Schwarzbek (Ohio State) next presented a paper on the influence of variable intercyclic intervals on pursuitmeter learning, and a new method of treating learning data. By testing 12 groups of a total of 86 *Ss* with the pursuitmeter he came to the conclusion that some theories of learning wrongly assume an original reenforcement of correct responses with a weakening of this tendency in the course of time. His experiments indicated that under these conditions there might have been an optimal interval of rest between arbitrary practice-units as much as 48 hr. in length. C. R. Griffith (Illinois) discussed a similar problem of maturation under the tital of "The Phenomena of Off-season Learning." By measuring in successive years the initial, maximal, and final skills of basketball players, tennis players, baseball players, and golfers, he concluded that the phenomena of off-season learning do not exist and that the increased efficiency after a lapse of time in learning may be of the nature of better judgment rather than of increased technical skill. This section was concluded by the report of an experimental study of Thorndike's theory of learning by M. Hayden (Wisconsin). She used 4 procedures including the one advocated by Thorndike in order to investigate other factors, besides the satisfaction of *E*'s comment which influenced improvement in efficiency.

A parallel section, over which J. R. Kantor (Indiana) presided, was opened by a paper on the relative rôles of wave-phase and acoustic intensity in sound-localization offered by O. C. Trimble (Purdue). By studying the effect of the respective factors with tones ranging from 125–7500 d.v. with 4 trained *Os* he discovered that localization on the basis of intensity is more immediate and definite than on the basis of phase. Phase is adequate for tones up to about 1250 d.v. This predominance of intensity over phase is effective even when the former is opposed to the latter. In the absence of E. Lindermann (Iowa) his paper on the distortion of space perception produced by *Cannabis Indica* was read by C. A. Ruckmick (Iowa). Eight out of 10 *Os* showed changes in perception which incorporated a dynamic relationship between the perceptual changes and the state of activity prevailing in other parts of the motor and sensory equipment of the individual. His findings discredit any isolated reflex theory involving the central nervous system on the basis of inhibition from higher centers when affected by toxic agents. H. F. Schick (Central Inst. for the Deaf) next presented a paper on a new interpretation of the action of the sweat-glands in connection with electrodes during experiments on affective responses. By means of diagrams the supposed contraction of the gland with its accompanying ejection of excretion in varying amounts was made responsible for the changing electrical resistances recorded. This paper was defended by M. F. Meyer (Missouri). A paper, by L. S. Woodburne (Michigan), on the effect of the visual angle on relative depth perception followed. In it the author questioned the classical assumption that the size of the object fixated was negligible in tri-dimensional perception. His apparatus was designed so that the size of the object could be made the same in appearance no matter what the actual distance from the *O* was. From 5000 judgments he concluded that the size of the visual angle certainly did not give a necessary cue in the

accurate perception of relative depth. R. F. Wallace (Ohio State) then showed the effect of the distortion of stroboscopic figures due to the relative velocities of the stroboscopic figure and the slit that moved before the eye He. proposed and checked a formula giving a determination which correlated 0.996 with the empirical situation. The last paper in this section, on visual acuity and illumination, was given by W. W. Wilcox (Kansas). By devising an apparatus exposing bright bars on dark ground and dark bars on bright ground and providing for an adjustment of intensity and the physical widths of the bars, acuity was carefully measured. This theoretical curve showing variation of visual acuity with intensity closely agreed with experimental results.

The fifth session opened on Saturday morning under the chairmanship of H. Carr (Chicago) with a paper by H. F. Harlow (Wisconsin). By means of motion pictures he illustrated the fact that social facilitation of feeding in the rat and in the Rhesus monkey was independent of learning. A paper on comparative tests on a human and a chimpanzee infant of approximately the same age by W. N. Kellogg and L. A. Kellogg was also demonstrated by motion pictures. In eight different situations the comparative initial response and rate of learning were traced in the chimpanzee and human baby during four and one half months of growth in the home under exactly identical conditions. The chimpanzee was 7½ mo. old at the beginning of the study and the human baby 10 mo. old. The human did better in delayed reactions and in detouring. In the "cap-on-head" test the ape showed marked superiority. There were also distinct differences in reaction to ice and to rotation. P. E. Fields (Stanford) then reported some visual discriminations obtained with a new multiple type jumping apparatus. A number of patterns were exposed to 84 white rats in 165,000 trials. Figure discriminations were made much more rapidly than in the usual type of apparatus because the multiple form of presentation forced the rat to use visual stimuli to the exclusion of other sensory channels. The functions of the superior colliculi in vision were discussed by J. D. Layman (Chicago). By destroying the superior colliculi the effect upon the discrimination of light from darkness and of various types of patterns was studied. He found that the extensive lesions made did not destroy visual capacities tested. There was some defect, however, in visual functioning as indicated by the slower formation of habits and in the hesitancy of the rats to jump. K. S. Lashley and L. E. Wiley (Chicago) noted the retardation from cerebral lesion in mazes differing in complexity. One hundred and twenty-seven operative animals and 60 normal animals were used. No difference was found in the relative difficulty of the mazes for animals with slight and those with extensive lesions, but a degree of retardation was evident and was best described as an exponential function of the extent of lesion. H. G. Swann (Chicago) described an apparatus for olfactory discrimination in rats, and discussed the reaction of rats to a variety of odors under visual, tactile, and auditory handicaps. While it was difficult to extirpate the olfactory lobes completely, of the 23 operative animals only 5 failed to show discrimination. There was a complete loss of habit, however, followed by slow relearning, indicating that olfactory cues were significant. The last paper was contributed by E. S. Girden (Illinois) on cannibalism in dogs. In five of eight tested animals aversion for raw dog flesh was shown but was not permanent. When dog meat was cooked no animal showed aversion to it.

The sixth section of papers running simultaneously with the above was presided over by A. R. Gilliland (Northwestern). It opened with a paper by E. B. Greene (Michigan), who tried to secure measures on relative human efficiency which should compare individuals in terms of method, motive, and fatigue. The results from various age groups showed a marked difference between easy and hard tasks which were explained through subjective reports that indicated very little change in fatigue by numerous changes in motive and method of application. The lowest quarter of students changed more and showed more improvement than the highest quarter on the easier tasks. But the reverse was true for the harder tasks. M. F. Meyer (Missouri) next read a paper on "Oberlin Grades Its Students in the Registrar's Office." He traced the various stages of growth in the manner of marking students but deplored the unfortunate retrogression recently in evidence at Oberlin whereby for purposes of transferring credits to other colleges a ranking order has been converted into percentages. D. Beise (Ohio) discussed the reliability and validity of certain physical tests compared with the reliability and validity of mental tests. She statistically examined 4 tests usually given in the gymnasium and disclosed the fact that these tests lacked the criteria of validity possessed by such mental tests as the Stanford revision of the Binet series. She concluded that tests which depend largely upon the cerebro-spinal nervous system for their responses show a higher measure of reliability than do those whose responses are governed by the autonomic nervous system. In a paper on the effect of pre-school attendance upon the I.Q., B. E. Wellman (Iowa) showed that preschool attendance increased the intelligence test scores. There were greater gains over two years of attendance than over one year; and over three years than over two years. Gains took place during the winter months while they were in school but not during the summer months when they were on vacation. The final paper, by F. P. Robinson (Iowa), on the rôle of eye-movement habits in determining reading efficiency showed that an increase in the efficiency of eye-movements also tended to increase the reading ability of poor readers. The groups were matched for intelligence, reading ability, class, and sex, which led to the conclusion that the resultant gains could only be attributed to the factors involved in the training technique.

Under the chairmanship of C. A. Ruckmick (Iowa) a 2-hr. session was arranged for reports from the various laboratories. A new procedure called for reports from the research workers themselves and under a limited schedule for presentation and discussion. Reports were ready by J. Pessin (Wisconsin), "Effects of social stimulation on human maze learning;" R. C. Davis (Indiana), "An investigation of the loss and acquisition of emotional reactions;" P. T. Young (Illinois), "Changes in food preferences of the white rat;" H. H. Remmers (Purdue), "Affective selection and liberalizing influence of college courses;" H. Woefle (Illinois), "Attitude toward intelligence tests;" D. L. Bidwell (Ohio State), "An experimental study of the learning and retention of paired associates;" R. K. Compton (Illinois), "An experimental and analytical study of organic set;" Z. Peel (Arkansas), "Similarity of form of interpolated learning and retroactive inhibition;" T. W. Forbes (N.Y. Psychiat. Inst.), "A study of sleeping postures of catatonics;" R. W. Husband (Wisconsin), "Comparative motor abilities of athletes and non-athletes;" D. P. Boder (Lewis Institute), "Some electronic devices;" W. F. Book (Indiana), "Learning studies;" G. Finch (Illinois), "The comparative auditory acuity of

dogs and human subjects." At the close Dr. C. E. Spearman was invited to discuss his coöperative research, which he is conducting this year at the George Peabody College for Teachers in an effort to bring together under a simplified mathematical formulation various types of mental measurements. He is seeking to prepare the way toward a mutual reconciliation of controversial points of view. M. F. Meyer also demonstrated a mechanical device which proved that a flexible artificial lens could change its focal length.

University of Iowa CHRISTIAN A. RUCKMICK

Anton Abram Grünbaum: 1885-1932

Grünbaum turned definitely to the study of psychology as his chosen field while he was a student at Würzburg in 1906. For three semesters he worked under Külpe and presented as his dissertation for the doctor's degree a detailed investigation of the contents and forms of abstraction.[1] He came to Würzburg at the time when Külpe's interest in the experimental study of higher mental processes was at its height, and was able to profit from association with an unusual group of men and women devoted to the working out of their director's ideas. In his own problem, for example, Grünbaum had as observers, among others, Külpe himself, Dürr and his wife, Miss Martin, Michotte, Bühler, Katz, Pfeiffer, Segal, and Spearman. A few years before, Külpe had made his famous report at Giessen on preliminary results obtained in an abstraction-experiment. Mittenzwey, under Wundt, followed soon after with a long critical, historical, and experimental study of the same topic in which the methods and conclusions of Külpe were treated with scant respect. Grünbaum's monograph served both as a reply to Mittenzwey and as a completion of Külpe's own results, with the addition of evidence regarding the abstraction and perception of items in a specially determined relationship. The work appeared in the same number with Bühler's reply to Wundt's attack on the introspective technique used at Würzburg.

The influence of Würzburg is apparent in nearly all of Grünbaum's publications during the next twelve years. Only one or two articles do not fall within the field of the higher mental processes or of sensory and perceptual phenomena. In 1916 he went from Bonn, where he had been assistant in the psychological laboratory under Störring, to Amsterdam. In 1922 he was made director of the psychological laboratory at Amsterdam, and was also closely connected with the work in the psychiatric and neurological clinic in the same city. This latter activity was beginning to make itself evident in his more recent publications.[2] Studies in thought, memory, abstraction, and determination gave place to work

[1] Über die Abstraktion der Gleichheit: ein Beitrag zur Psychologie der Relation, *Arch. f. d. ges. Psychol.*, 12, 1908, 340-478.

[2] Publications later than those listed in *The Psychological Register* follow: Experimentelles zur Lehre vom systematischen Zusammenhang der motorischen Erscheinungen, *Zsch. f. d. ges. Neur. u. Psychiat.*, 120, 1929, 286-340; (with Q. Snapper) Lactic acid eliminated in urine and sweat during various games, *Biochem. Zsch.*, 208, 1929, 212-220; The pointing position of the hand as a pathological and primitive reflex, *Brain*, 53, 1930, 267-277; (with L. Bouman) Über motorische Momente der Agraphie: Schreibstörungen und Zwangsgreifen, *Monatssch. f. Psychiat. u. Neur.*, 77, 1930, 223-260; Aphasie und Motorik, *Zsch. f. d. ges. Neur. u. Psychiat.*, 130, 1931, 385-413.

on reaction-times, psychogalvanic reflexes and their diagnostic value, aphasia' various aspects of motor disturbances, child psychology, and, far removed from the influence of Würzburg, the behavior of spiders. Grünbaum wrote chiefly in German, although several articles appeared in French and in English. His untimely death occurred in Amsterdam on the tenth of January, 1932.

Harvard University CARROLL C. PRATT

Leonard Thompson Troland: 1889-1932

Leonard Thompson Troland, whose death occurred May 27th, 1932, was one of those rare individuals who can follow two distinct careers at the same time and be successful in both. He studied psychology at Harvard University under Münsterberg, Holt, and Langfeld from 1912 to 1915, obtained his doctorate from Harvard in the latter year with a thesis on visual adaptation, and carried on his work in vision during 1915-16 as Sheldon Fellow at the Nela Research Laboratory in East Cleveland. In September, 1916, he was appointed Instructor in Psychology at Harvard, and he continued to teach psychology there until the time of his death. Besides a host of articles on psychological problems—mainly problems of vision—he published three major works in psychology: *The Mystery of Mind*, a rather popular introductory text (1926); *The Fundamentals of Human Motivation* (1928); and *The Principles of Psychophysiology*, a general treatise in four volumes of which only the first three had been published at the time of his death (1929-32).

Parallel to this work in psychology, however, ran a well-filled career in theoretical and applied physics. As an undergraduate at the Massachusetts Institute of Technology, from which he received the degree of S.B. in 1912, Troland took a number of courses in optics and in theoretical physics under D. F. Comstock. In 1917, he collaborated with Comstock in publishing *The Nature of Matter and Electricity*, an introductory exposition of the basic concepts of modern physics. The following year he became research engineer for Kalmus, Comstock and Wescott, Ind., and began with Comstock, E. A. Wever and J. A. Ball the collaboration which was to make him co-inventor with these men of the Technicolor Process for the production of colored motion pictures. His work upon the Technicolor Process continued with Kalmus, Comstock and Wescott, Inc., until 1926, and thereafter, until the time of his death, with Technicolor, Inc. A measure of his contributi to optics is afforded by his election, in 1922, to the presidency of the Optical Society of America.

It is undoubtedly his interest in physics which was the most important factor in shaping Troland's psychology. True, philosophy played an important part in his life. As a graduate student at Harvard he studied Greek philosophy with J. H. Woods, ethics with R. B. Perry, epistemology with Perry and Bertrand Russell, logic with Josiah Royce, and philosophy of nature with E. B. Holt. He held a definite philosophical position, a form of psychical monism tempered by acceptance of physico-chemical explanations at the scientific level, and contributed a number of articles on philosophical issues.[1] Philosophy, however, did not

[1]The chemical origin and regulation of life, *Monist*, 24, 1914, 92-133; Philosophy and the world's peace, *J. Philos.*, 13, 1916, 421-437; Paraphysical monism, *Philos. Rev.*, 27, 1918, 39-62; The significance of psychical monism, *Psychol. Rev.*, 29, 1922, 201-211; Psychophysics, the key to physics and metaphysics, *J. Wash. Acad. Sci.*, 12, 1922, 141-162.

affect his psychology except as it provided a setting. In treating a psychological problem he was indeed wont to emphasize its philosophical connections in the introduction and in the conclusion, but his main concern was always to formulate exact functional relationships between quantitative variables. The ideal towards which he was ever striving was a psychology patterned after modern physics, a psychology made up of mathematical equations.

This ideal accounts largely for his many excellent contributions to visual psychophysics. In the first place, it determined him to concentrate his experimental work in vision, the sense-department in which control and measurement of the stimulus has unquestionably reached the greatest development. In the second place, it determined him to select within his chosen field problems which were not merely soluble, but whose solution could be stated in the unambiguous language of mathematics. His doctoral thesis, for instance, dealt with visual adaptation, in which time-exposure could be related mathematically to wave-length, intensity and area of stimulation.[2] Again, his subsequent experimental contributions include work on the relation of the duration of a negative after-image to that of the exposure producing it,[3] of flicker-photometer frequency to the color and intensity of the alternated lights,[4] and of the differential threshold for brightness to color differences between compared fields,[5]—all of them problems leading to mathematical functions. Indeed, results not subject to mathematical description were usually considered by Troland to be merely preliminary in nature.

It is also this ideal which accounts for the extreme definiteness and clarity of his psychophysiological theories. As early as 1914—a year before receiving his doctorate—Troland voiced his faith in what he called the "method of mathematical hypothesis,"[7] the method by which processes are described in terms of equations developed from a limited number of simple mathematical postulates. In the previous year he had already applied this method in presenting a physico-chemical hypothesis to explain visual response.[8] He applied it again, in 1914, in

[2]The thesis is *Studies in Visual Equilibria, Harvard University*, 1915. Cf. The colors produced by equilibrium photopic adaptation, *J. Exper. Psychol.*, 4, 1921, 344-390.

[3]The laws of visual minuthesis: the threshold pre-exposure time and the equilibrium time for a projected negative after-image, *J. Franklin Inst.*, 181, 1916, 579-181.

[4]Notes on flicker photometry: Flicker-photometer frequency as a function of the color of the standard, and of the measured, light, *ibid.*, 181, 1916, 853-855; Flicker-photometer frequency as a function of light intensity, *ibid.*, 182, 1916, 261-263.

[5]The heterochromatic brightness discrimination threshold, *ibid.*, 182, 1916, 112-114; The heterochromatic differential threshold for brightness, I. Experimental, *Psychol. Rev.*, 25, 1918, 305-329; II. Theoretical, *ibid.*, 359-377.

[6]The absence of the Purkinje phenomena in the fovea, *J. Franklin Inst.*, 182, 1916, 111f.; The reversal of a faded negative after-image by brightening the stimulus field, *ibid.*, 529 f.; Preliminary note: the influence of changes of illumination upon after-images, this JOURNAL, 28, 1917, 497-503.

[7]Adaptation and the chemical theory of sensory response, this JOURNAL, 25, 1914, 525.

[8]A definite physico-chemical hypothesis to explain visual response, *Amer. J. Physiol.*, 32, 1913, 8-40.

formulating a general mathematical theory of adaptation[9] and in 1918, in giving a mathematical interpretation of his data on the heterochromatic brilliance threshold.[10] It is true that his theory of audition,[11] so well in accord with the results of Wever and Bray, can hardly be termed quantitative. Even in this case, however, the "method of mathematical hypothesis" shows its influence in the choice of fundamental relationships readily subject to quantification. The result of this faith in mathematical formulation is that Troland's sensory hypotheses conform well to the standards of good scientific theorizing: they are devoid of unintentional ambiguity; they account accurately—not just approximately—for available facts; and they frequently imply—not merely suggest—further observable, but as yet unobserved, facts and are thus capable of real verification.

Troland's application of the "method of mathematical hypothesis" was not limited to sensory processes. In 1920, he propounded in mathematical terms a "system for explaining affective phenomena"[12] which was to become the nucleus of his subsequent volume on motivation.[13] On the surface this theory appears to have the same virtues as his sensory hypotheses. On the surface only, however. Consider the basic equation $a = k\,(dc/dt)$, in which a represents degree of affection, c the average conductivity of active cortical synapses, t time and k a constant. Does this relationship have any definite empirical implications? It surely would have many if only c were subject to measurement, but such is not the case—nor is likely to be the case for many decades. The "method of mathematical hypothesis," to be fruitful, requires the use of well-defined variables. In Troland's theory of affection it is of little more value than it was in the psychology of Herbart.

Familiarity with mathematical relationships is surely no warrant of expository ability. Yet in Troland there did seem to be a definite causal relationship between these two factors. Not only was he a clear writer and an erudite one, but he had an extraordinary knack of classifying facts under functional relationships in such a way that they constituted a logically and psychologically organized whole. It is this ability which distinguished his many surveys of the literature of vision, particularly *The Present Status of Visual Science*.[14] It is likewise this ability which made his *Principles of Psychophysiology* a truly great contribution, comparable with the like work of Wundt, of Ebbinghaus, and of Ladd and Woodworth. The three volumes published to date include many interesting and novel features, such as the definition of perception as the relation of consciousness to the stimulus object, and that of sensation as the relation of consciousness to the afferent arc. Their outstanding merit, however, is that they provide for the entire field of modern psychophysics and psychophysiology a source which, though adequate

[9] Adaptation and the chemical theory of sensory response, this JOURNAL, 25, 1914, 500-527.

[10] The heterochromatic differential threshold for brightness, *op. cit.*, note 5 *supra*.

[11] The psychophysiology of auditory qualities and attributes, *J. Gen. Psychol.*, 2, 1929, 28-58.

[12] A system for explaining affective phenomena, *J. Abn. Psychol.*, 14, 1920, 376-387.

[13] *The Fundamentals of Human Motivation*, 1928.

[14] The present status of visual science, *Bull. Nat. Res. Council*, 5, 1922, 1-120 (no. 27).

for the commencement of actual research, is yet clear enough to be mastered without special preliminary training.[15]

Troland's death will be regretted by many psychologists, who, familiar with his work in vision, appreciated the fruitfulness of his mathematical approach and the excellence of his careful experimental technique; by his students, whether auditors in the classroom or readers of his works, who were rarely disappointed when they turned to him for the elucidation of some particularly complicated issue; and by his friends, who benefited not only by his intellectual acumen, but by his generous personality.

Harvard University J. G. BEEBE-CENTER

AN ACKNOWLEDGMENT

The editors of the JOURNAL and its readers are indebted to Professor H. P. Weld of Cornell University for the portrait of Wilhelm Wundt, which appears as the frontispiece of this number. The photograph was made in 1909, during Wundt's seventy-seventh year. The signature, in German script, which appears beneath the portrait, is a facsimile of the signature on the photograph.

K. M. D.

ERRATUM

Due to an oversight in proof reading the error in the placement of the line cuts in Fig. 1, in my note on "The Method of Gestalt Psychology," appearing in the April issue of this JOURNAL (*supra*, 357), was not detected. Each of the zinc blocks in Fig. 1 was mistakenly rotated through an angle of 90°. The figures, as stated in the text of the note, were duplicates of Koffka's and were used in the experiments reported in the note in the exact form and position used by Koffka.

Oklahoma College for Women HOWARD TAYLOR

[15] *The Psychological Register*, III, 1932, 503-505, gives Troland's bibliography, fifty-seven articles or volumes. The following titles should be added to this list in order to secure approximate completeness: A rejoinder to Drs. Ferree and Rand's note, *Psychol. Bull.*, 17, 1920, 136-142; Vision, general phenomena, *ibid.*, 201-228; The progress of visual science in 1919, *Amer. J. Physiol. Opt.*, 2, 1920, 232-268; Henri Piéron on the physiological principles underlying the study of light, *Trans. Illum. Eng. Soc.*, 16, 1921, 23-48; with C. H. Langford, The interrelations of brilliance and chroma studies by the flicker technique, *J. Opt. Soc. Amer.*, 6, 1922, 409; with Langford, The stability of hue under chromatic minuthesis, *ibid.*, 10, 1925, 294.

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

Minnesota Mechanical Ability Tests. By D. G. PATERSON and R. M. ELLIOTT, directors of the investigation; L. D. ANDERSON, chief investigator; H. A. TOOPS, statistical consultant; and EDNA HEIDBREDER, editor of the report. Foreword by R. M. Yerkes. Minneapolis, University of Minnesota Press, 1930. Pp. xxii, 586.

This is the report of an investigation subsidized by the Committee on Scientific Problems of Human Migration of the National Research Council. Yerkes, who was chairman of this Committee in its early history, traces in the Foreword the origin and purpose of the research. The work was carried out in the University of Minnesota with the aid of numerous assistants. The authors say that the members of the subsidizing committee "were generous to the point of indulgence with their encouragement and financial backing (p. ix), so it is presumable that a fair opportunity existed to try out the concept of 'mechanical ability' (or abilities). The term is used in this research "to refer to whatever capacities and abilities are necessary for certain kinds of work—specifically, work that involves the manipulation of tools, the operation of machinery, and the planning and execution of pieces of work which involve these and similar activities" (p. 5). The practical purpose was to construct a scale of tests of mechanical ability in the sense indicated in the quotation. Providing for proper motivation is no small part of such test construction.

If "maximum motivation" is obtained in the tests, can one from these data predict direction and degree of success under conditions of motivation in actual mechanical work? It was assumed that a battery of tests would be better for this purpose than a single test, and that the tests constituting this battery must be reliable and correlate as highly with the criteria as possible and only little or not at all among themselves. To secure these tests and criteria required considerable preliminary experimentation and a large number of subjects for different tests.

In later experimentation it was found that several tests of low reliability could be made consistent in their measurements merely by a suitable lengthening of the tests. To ascertain effects of such lengthening, three different applications of the preliminary tests were given. The subjects in this preliminary experiment were 217 junior high school boys, each being tested 11 hours on group and individual tests, including a retest series after a seven-day interval. The boys' success in five different shop courses was determined on the basis of their teachers' judgments by means of the rank method. Rank scores in the several courses were all converted into σ scores and averaged to obtain "the combined shop criterion." This criterion was found to give positive but low correlations with all 29 of the tests used (including intelligence tests); only two of these correlations were above 0.40 (Link's Spatial Relations, longer board, time being 0.44 and the Paper Form Board, 0.43). Seventeen of these 29 correlations were greater than four times the P.E., or than 0.15. By the Toops multiple-ratio technique a battery of seven

tests was constructed, which gave a correlation of $0.60 \pm .03$ with the criterion, of $0.07 \pm .05$ with intelligence scores, and low inter-correlations. Reliabilities obtained by means of a retest 7 days later were not satisfactory but it was found that the defect could be largely removed by a proper lengthening of the tests.

The work thus far done was preliminary. During the academic year 1924-25 the experiment proper was carried out on a new set of 150 boys then entering the Jordan Junior High School. The following improved tests were used, a great number of the earlier tests having been discarded: The Minnesota Assembly tests (after Stenquist); the Minnesota Spatial Relations tests (after Link); the Minnesota Paper Form Board tests (after Army Beta); a card sorting, a packing blocks, and a nine-hole steadiness test; and the Stenquist Mechanical Aptitude Picture Tests, I and II. Age, Otis S-A Higher Examination scores, academic success, experience in mechanical work, interests, motor ability (agility and gymnasium success), anthropometric measures (height, weight, vital capacity), social and economic status, and home influence—all determined by as objective means as possible —were studied in their relations to success in the tests. The third and fourth of these factors were determined by means of questionaries and interest-analysis blanks, respectively. The Otis test was preferred to Alpha "because of its simpler timing and scoring methods," and because in the preliminary experiments the inter-correlation of these tests "was very high" (p. 113). From Table 88 (pp. 432-433) this correlation is found to be $0.74 \pm .02$, which gives a predictability of the one score when the other is known of only 0.33. The reviewer has for years obtained considerably better predictability of success in psychology examinations with Alpha than with Otis scores, and he doubts the wisdom of the use of the latter instead of a revision of the former test, despite the greater convenience of the Otis test.

The criterion of mechanical ability—the standard against which all the tests are rated as to validity—was also much improved in the experiment proper. A long chapter (Chapter 9) of 58 pages describes fully the construction and nature of this somewhat complex criterion, giving rating scales and other devices for measuring the degree of excellence of different kinds of shop work in the schools, test blanks on mechanical information and on training. These rating scales are in terms of carefully ranked samples of completed work, reproduced in half-tone. Several criteria of different activities—each usable separately for its particular kind of shop work—were thus produced, and since there was no single rating or ultimate criterion with respect to which these could be severally weighted, it was necessary to convert the scores into standard units and to combine them by weighting equally the different units (in the form of weighted averages), except that the quality-quantity criterion was the combined scores of the quality and the amount of all shop work done, not divided by the number of projects completed (p. 202).

In the various stages of the combination of different and more special criteria, the construction of the final criterion of mechanical ability in general seems to have been based on the assumption that all the different tests contributed alike to measuring some sort of general, unique mechanical ability trait. It can hardly be contended, however, that any such trait has been established. A battery of tests of some more or less vague mechanical ability has been constructed, and the scores on different elements so combined as to correlate more highly with success in shop work in the schools than with intelligence tests. That seems to be about all that

can be inferred from the work as to the real nature of mechanical ability. It may be noted, moreover, that in converting raw scores into scores for equal weighting, this purpose to weight equally is hardly fulfilled unless the σ of each set of test scores is corrected for unreliability; since the size of the σ is a function of its unreliability. The error here, however, is not great, since each test was made as reliable as practicable and low reliability of any test would have a tendency to underweight it in the combined score. In the last part of the book (pp. 309-586) are to be found detailed directions for administering and scoring the battery of tests finally developed; statistical methods; correlation tables; percentile norms, and other valuable data, as well as original scores both from the preliminary experimentation and the final experiment proper; a classified bibliography of 17 pages; and an excellent index, covering 25 pages.

The work on establishing criteria and percentile norms of mechanical abilities, on preparing objective tests and scales, on organizing the rating and the test-scales and results for practical use is of considerable value; and the tables of original data will enable other workers to check and evaluate the methods and interpretations. In view of the excellent coöperation under most favorable school conditions and of the technical assistants employed, the work done was so stupendous and excellent in details that future mechanical ability determinations will probably be dependent to a considerable extent on this publication for many years. It is therefore of practical importance that all the items of the assembly tests may be available to testers for some time, and that their uses be clear. Item 10 of Box A and items 8 and 10 of Box B (pp. 320 and 323) are marked "not included in the present series." Were scores on these tests included in the data on which norms were established? This isn't made clear.

One is struck by the enormous number of tests that had to be dropped out because of small value. Would a more careful survey of past work not have saved much time, or was this extent of "trial-and error" essential? The practical-minded reader would doubtless like to know more about the time for administration and the scoring time of the tests. Much of the scoring seems to be of a laborious nature. Certainly reliability coefficients listed as obtained from the combined estimates of ten judges (Table 20) are impracticable in actual use, and the coefficients obtained by the up-step Spearman-Brown formula by the odds-evens device will be found too high in actual practice, where the perfectly controlled conditions assumed by the formula cannot be realized. No data yet available indicate the relationships of success in the tests and success in practical mechanical work outside the schools.

It must be said, finally, that the work under review is a conspicuous example of elaborate and carefully executed work of high quality, carried out coöperatively by many workers of both scientific and practical interests. J. P.

Genetic Studies of Genius, Vol. III. The Promise of Youth. By BARBARA STODDARD BURKS, DOROTHA WILLIAMS JENSEN, and LEWIS M. TERMAN. Stanford University, Stanford University Press, 1930. Pp. xiv, 508.

This volume consists of the follow-up studies of a thousand gifted children first tested in 1921-22. The authors sought answers to two fundamental questions concerning the development of gifted children: (1) "How well does the mental

ability of these gifted children hold up on the average? (2) How many of the children later show radical changes in intelligence rating, either positive or negative?" Methods and results are presented in Part I. The tests used comprise 5 standard intelligence tests, 5 achievement tests (besides school marks), 6 personality tests, including tests of emotional stability, femininity-masculinity, interest, etc., and 9 information blanks sent to parents and schools. The results of retesting in 1927-28 show a drop in IQ for both sexes of 9 points, 3 points for boys and 13 points for girls. Some drop in IQ is to be expected statistically as due to unreliability of measurement. The children who were first tested at an early age do not show a greater drop in IQ than those who were first tested at an older age.

No evidence was discovered to indicate that such factors as environment, personality, health, race and the nature of the Stanford-Binet test were significant in causing marked IQ changes in gifted children. No significant differences between early and late testing were apparent with respect to interest in school, achievement in school subjects, regularity in school attendance, personality traits (*e.g.* fair-mindedness and masculinity-femininity), and social intelligence.

While no direct comparison could be made with respect to the general health of gifted children between the follow-up group and the 1921-22 group, the reports obtained from information blanks tend to disprove the popular belief that gifted children are apt to be deficient in health and physical vigor. The reports from home and school show that from 77-90% of the gifted boys and girls have good general health and that from 1-5% have poor general health. However, the number of girls who show symptoms of general weakness is nearly twice as great in 1927-28 as in 1921-22. In the follow-up group the school blanks report 15% of the gifted boys and 18% of the gifted girls as showing symptoms of general weakness. It was found that gifted children sleep somewhat less daily in 1927-28 than in 1921-22, but they still sleep more than untested children.

All but two of the 1921-22 high school group who coöperated in the follow-up have graduated from high school, and more than 90% of both boys and girls have gone, or plan to go, to college. Approximately one-half of those graduating from college have taken postgraduate work.

Most of the boys and two-thirds of the girls who have graduated from college have decided upon their life vocations. Gifted boys who have graduated from college tend to choose the professions, while non-graduates in the gifted group are more likely to choose a business vocation.

Part II deals with case studies of youthful geniuses, gifted leaders, musicians and musical composers, artists and devotees of science. These individual studies reveal certain inconsistencies in development. A few cases of marked or moderate decrease in IQ and achievement are reported, but a large majority of the individual cases studied show consistent development during the period between 1921-22 and 1927-28.

Part III is a study of literary juvenilia, the object of which was "(1) to derive a scale with steps high enough to measure the best writings of the most talented children, (2) to compare the productions of California gifted children with the juvenilia of eminent writers, and (3) to study the environment and early development of those who have evidenced precocious literary ability." Fourteen California gifted children with IQ ranging from 140 to 188 (ages ranging from 6 yr. 9 mo. to 13 yr. 10 mo.) were found to possess outstanding literary ability. The

literary productions of these children compare favorably with the juvenile productions of 28 English and American men of letters who were rated as having IQ of 140 or over.

Part IV gives a summary, and an appendix presents the protocols which were used in securing information from parents and schools.

The chief value of the follow-up studies is suggested by the title of the present volume, "The Promise of Youth." Although the reliability of some of the tests is still in doubt, the combined results point unmistakably to the conclusion that "the gifted individual whom intelligence tests may discover in childhood" will become "the extraordinary genius who achieves the highest eminence."

University of Montana FRANKLIN O. SMITH

The Treatment of Behavior Disorders Following Encephalitis: An Experiment in Re-Education. By E. D. BOND and K. E. APPEL. New York, The Commonwealth Fund, Division of Publications, 1931. Pp. 63.

In this small book, an account is given of an experiment in the treatment of the disorders of behavior which follow encephalitis. The first chapter contains a clear and non-technical account of the disease of encephalitis. Case histories are then presented which graphically illustrate the unfavorable changes in behavior which often develop as an aftermath of the disease. Previously normal, well-adjusted children after the disease frequently show marked behavior disorders and social retrogression.

The experiment concerning which the book is written, deals with the organization of a hospital-school under the direction of the chief psychiatrist of the Department of Mental and Nervous Diseases of the Pennsylvania Hospital. The work in the school consists in an ingenious effort to reëducate the total personalities of children who are in its charge. A balanced program of sleep, work, and directed play has been organized, which occupies the whole twenty-four hours of each child. This program, and its sympathetic and personal application, developed in the light of individual differences, may well be studied by anyone who is interested in modern methods of changing atypical behavior in children. The results of the experiment show that, in a substantial number of cases, the treatment has led to real improvement.

Approximately one page of the book is given to a section headed "Psychological Theories." Here the rather startling statement is made that, "In treating the children in the hospital school, if a theory appeared to throw light on a problem, that theory served as the basis of treatment, whether it implied the behaviorism of Watson, the inferiority feelings of Adler, the 'needs' of Thomas, the type-reactions of Jung, or the psychological processes emphasized by Freud." Is it too much to hope that the time may come when psychiatrists may recognize psychology as a scientific and useful body of fact and not as a welter of conflicting views in regard to personality?

Save for the criticism suggested above, the book seems well calculated to supply a significant program for those who are concerned in dealing with the distressing after-effects of encephalitis.

Brown University LEONARD CARMICHAEL

Hypnotic Poetry. By EDWARD D. SNYDER. Foreword by James H. Leuba. Philadelphia, University of Pennsylvania Press, 1930. Pp. xi, 162.

The author, an associate professor of English at Haverford College, suggests that certain poems are largely "spellweaving" poems which influence the auditor in a fashion different from the influence of more "intellectualist" poems. The "spellweaving" poems are capable of stimulating the listeners to various states, from highly emotional to mildly trance-like, and this without the poems having a "rational" appeal. It is suggested that there is a close parallelism between the results of such poems and the phenomena of hypnosis, including post-hypnotic suggestion; and between the factors producing both the "spell" and the hypnosis. Among these factors are: monotonous rhythm of auditory stimuli; freedom from abruptness; vagueness of imagery; and slight fatigue due to minor complexities. The relationship between these states and phenomena of religious revivals, due largely to "hypnotic hymns," is pointed out. Certain implications bearing on literary criticism are mentioned.

The book is not a text on poetics or on hypnotism, but it is intelligently written, and suggests certain applications of psychological knowledge which are worth considering by those interested in the phenomena of hypnotism or in poetic literature.

University of Pittsburgh J. H. MCFADDEN

Foundations for Human Engineering. By C. R. Gow. New York, Macmillan Co., 1930. Pp. xiii, 226.

This book presents briefs of lectures constituting a course in "Humanics" which is elected by seniors at the Massachusetts Institute of Technology. The general purpose seems to be that of formulating an applied science of self or character development. One obvious weakness of "Humanics," as presented, is that it seems to have much less basis in psychology than does mechanics in the science of physics. The foundation of "Humanics" is only the superior common-sense of the author, who is an experienced and successful business man with a frankly commercial point of view.

The text is essentially a discussion of the virtues from the point of view of a paternalistic capitalist. It contains a long list of *do's* and *dont's* for the guidance of the young man who desires, above all things else, to be "successful" under the sacrosanct sway of "business." Honesty and loyalty are among the chief virtues. Honesty is preferred, apparently because it is the best policy. Loyalty is likewise indispensable. "As long as one is supposed to serve an employer and accepts pay for that service, let him be *for* that employer in every honorable way,"etc. Other virtues are tact, patience, courage, cheerfulness and friendliness. On the other hand "one of the chief obstacles to progress and prosperity is anger." Other vices are conceit, egotism, envy and criticism. "By all means do not criticize others. Praise them if you can do so." Many striking illustrations of the advantages and disadvantages of these traits in business are given. Practice to develop traits, as specific entities, is urged. "Training in observation should begin early." Abstractions are interpreted in terms of other abstractions.

Credit must be given the author for his sensible and unspoiled, albeit rather naïve, attack on the problem of character development in a technical school.

Some excellent advice is given in regard to the discovery and cure of specific defects of personality, such as peculiarities of voice, gesture, and expression, but the book does not have a scientific point of view or background. Furthermore the accepted standards of good character are evidently specified by the same people who write the specifications for the other products of engineering, such as skyscrapers and automobiles. Too much standpat conformity to a fixed moral code may not be a good thing in the long run, even in business.

University of Colorado THOS. H. HOWELLS

Voice and Personality. By T. H. PEAR. London, Chapman and Hall, Ltd., 1931. Pp. 247.

This book touches upon problems of the voice as an expression of personality. The first half is devoted to a discussion of 'dialect' and local differences in speech or accent, and the last half to a description of experiments carried on by the author and his assistants.

The experiments suggest many problems of interest to psychologists. Examples of these are as follows: How is the judgment concerning any particular attribute of the voice formed in the "wireless" listener's mind? How would a listener who does not understand our language be affected by these attributes? What effect will broadcasting have upon standardizing the speech of different localities and of the different professions? Wherein may psychological analysis aid in discovering and remedying the defects in the voice that fails to "get across." The volume on the whole is an excellent piece of popularizing work, and is extremely valuable as suggestive of fields for further investigation.

University of South Carolina W. F. SMITH

Psychology of Early Childhood up to the Sixth Year. By WILLIAM STERN. Translated by Anna Barwell from the 6th German edition. New York, Henry Holt & Co., 1930. Pp. 612, 11 pl.

The new English edition of this classic differs little from the earlier translation of 1924 (based on the enlarged 3rd German edition of 1923). The bibliography, however, is much extended, the text contains a number of references to recent research on Russian children, and the four plates of Kurt Lewin's film-photos of expression movements in children (which appeared first in the fourth German edition) have been appended to the illustrations.

The treatment throughout leans heavily on the psychographic studies of the Sterns, Scupins, Bühlers, Katzes and others; centering naturally about the socialized activities of drawing, speech, and the interpretation of pictures, with the investigation of which Stern and his wife have been so long identified. Ever since the third edition, however, the text has striven to take account of and to criticize the work and theories of Bühler, Koffka, Freud, Montessori, and Piaget as well.

The principle of convergence adopted by the author (*i.e.* the interaction of innate aptitudes and experience) absolves him from the painful necessity of disentangling hereditary and environmental influences. While the recurrent stress on the 'unity of personality' expressly implicates the total psychophysical unit, the usual sections on biology and neurology are conspicuous by their absence. References to the Watson-Blanton-Sherman type of observation, or to Russian

'reflexology' are rare. Much illuminating material, however, bearing both on the development of perception and on the emotional life of the child is made available in these pages. Aesthetic perception (in music, rhythm, and pictorial representation) and phantasy or imagination, are especially stressed. While the generalizations are based largely on children from social strata above the average, the work forms an invaluable supplement to the usual American text.

Cornell University ELSIE MURRAY

Child Psychology. By MARGARET WOOSTER CURTI. New York, Longmans, Green & Co., 1930. Pp. ix, 527.

This is a student's text, the fifth in the series edited by H. A. Carr. The language is for the most part non-technical, on the level of commonsense rather than of science (notably in the use of such words as 'idea,' 'thinking'). Hence the layman no less than the student will find it readable. Psychologists in general may also derive profit from its pages, since it offers a fairly impartial survey or digest of current and recent literature on the topics treated.

Writing from the standpoint of Carr's 'functionalism,' the author strives to avoid dualistic snags, and the extremes of a one-sided subjectivism or behaviorism. Mind is defined as the capacity for 'unified and effective adaptation to environment,' and space is assigned both to overt and to implicit (conscious or neural) response. The first hundred pages are largely biological, dealing with heredity, physical and mental growth, and native responses. The nervous system gets scant mention. The section on 'perceptual-motor learning' gives a detailed account of Russian work on children, including Krasnogorski's use of conditioning technique in the early diagnosis of individual types, as the author herself saw it demonstrated in Leningrad.

The term 'conditioning' is used consistently throughout the text, carrying often precisely the same connotation as the 'association" of the older schools. Though rejecting some of Watson's concepts, the author fails to build up for us a picture of the child as an individual, as does Stern, presenting us rather with a human mechanism or robot. A mechanism, moreover, with a bent toward going awry; for although her theme is avowedly the normal, Mrs. Curti's slant, in consequence perhaps of her clinical experience, is more often toward the abnormal. In her suggestive chapter on motivation, though critical of the Freudian stress on sex, she retains the stereotyped old rubrics—'compensation,' 'rationalization,' 'conflict.' Not only do these affix a pathological stamp to perfectly normal trends, they presuppose a view of human nature only a step above the Calvinistic or Hebraic. 'Conflict,' especially, in emphasizing the antiquated 'either-or' dilemma of contradictory opposites, ignores the complexities of modern community life. Even in childhood, surely, ethical problems center less upon the collision of code and inclination than on the difficult harmonization of several slightly compatible aims—vocational, recreational, and domestic.

The discussion of the origin, growth, and organization of meaning (defined not as an abstract entity, but as the totality of potential responses to a given situation) occupies another section, one of the best. While paying her repects to Carr and Dewey, the author follows in the main Piaget's illuminating use of *syncretism*, *juxtaposition*, and *egocentrism*, intracing the development of child thought. *Gestalt* nterpretation—except when pressed to extremes—is shown to be compatible

with current experimental findings; but reasoning is handled as essentially a trial-and-error process, a mental fumbling. The importance of *Einstellung* or set receives as little recognition as it does at the hands of configurationist or objectivist.

In the chapter on play, the author summarizes a number of recent American studies—mostly statistical—throwing them into perspective and relating them to standard theories. Finally, a hundred pages are devoted to discussion of the relative weight of defective intelligence, emotional instability, and social milieu in influencing delinquency and anti-social conduct. Burt, Healy, Slawson, Hartshorne and May are quoted. The book concludes with a short but sane chapter on the organic and environmental factors conditioning personality and traits (the latter defined as 'functional relationships distinguishable in a total of adaptive activity'). There are numerous footnote references and an index, but no adequately organized bibliography. A 'Work-Book' for students is said to be in preparation.

Cornell University ELSIE MURRAY

L'éducation fonctionnelle. By ED. CLAPARÉDE. Neuchatel, Delachaux & Nestlé, 1931. Pp. 267.

A ninety-seven page "Introduction" explains the theory and the sources of functional psychology traced historically from Locke and Herbart to the present day. Following this introductory material is a discussion of 10 laws of conduct to be employed in utilizing the child's activity, with special emphasis upon the laws of need and interest. Among the other laws there is some discursive overlapping. The next one hundred and forty pages include articles, many of which had already been published, on various functional aspects of education. The remaining twenty-four pages are devoted to a "Conclusion" answering the classic question, "Is education a preparation for life or life itself?" The author's final statement may be translated as follows: "In order to prepare for life education must be life. If education aims to be a preparation for life without being life itself it does not prepare for life."

Functional education is defined as that which considers the needs of the child and his interest in accomplishing his purposes. By the term interest is meant the factor which brings about adjustment in governing the individual's reactions and adapting them to those needs. To provide proper conditions for creating appropriate needs for which the child will seek satisfaction is the business of the school.

The volume contains no index beyond an alphabetical list of 114 psychologists and noted educators whose ideas are utilized in establishing and corroborating the author's theory of functional education. Of this number he quotes about thirty American writers.

Because the book is not arranged in conventionally numbered chapters it gives the impression of being fragmentary. Every section, however, reënforces the central idea of need and interest. Emphasis upon important points is obtained by well-chosen introductory statements and summaries. The style is non-technical.

The book contains little that is new. It presents, however, a definite point of view and brings to the attention of the reader most of the best that has been said concerning the aims of that much discussed and much defined process which we call education. The reader is left convinced that consideration of the child's real needs is fundamental.

Rhode Island College of Education GRACE E. BIRD

Diary of a Child's Life. By VILHELM RASMUSSEN. Translated from the Danish by Margery Blanchard. London, Glydendal, 1931. Pp. viii, 187.

This is a series of observations, kept systematically from birth to the fifteenth year, on the author's second daughter. It is presumably identical with *Et Barns Dagbog* printed in Denmark in 1922. The entries (made by either parent) range between 50-100 items yearly. They are set down in chronological order, usually dated, under such commonsense captions as attentiveness, catching hold, fear, recognition, temperament, standing, walking, language, sentences, animism, counting, time, rage, lies, bashfulness, sympathy, and religion, and are designed to stimulate the keeping of similar diaries by others. There are numerous reproductions of the child's drawings at different ages; also of photographs illustrating varied facial expressions and postures. The little volume affords a vivid picture of a developing child personality, and forms an interesting supplement to Rasmussen's earlier, more systematic series.

Cornell University ELSIE MURRAY

La personalité de William James. By MAURICE LE BRETON. Paris, Hatchette, 1929. Pp. 383.

This is a detailed, well documented, account of the hereditary and early environmental factors contributing to the personality and achievements of the famous psychologist and philosopher in whom began the third generation of the Jameses in America. The life of the psychologist's grandfather, also William James, who came to America in 1789, at the age of 18 years, with very little money and a Latin grammar, and who began in Albany as a clerk in a store and rapidly worked himself up to be a leading business man and banker, amply providing for his eleven children, is briefly sketched. There is also a good account of the father, Henry James, who left the Princeton Theological Seminary with a growing antipathy to all ecclesiasticisms and turned his back on the ministry to develop his own type of Swedenborgian philosophy; of his solicitude for the education in America and Europe of his five children; of William's uncertainty and vacillation in choosing his life's work, his experiences in Europe, his important contact with Agassiz on the trip to South America where he decided definitely to give his life to philosophy, but where he also learned to have respect for objective facts in his generalizations.

Le Breton makes a careful analysis and evaluation of James' contributions, indicating how they reflect the life and personality of a keen and serious-minded individual who had taken philosophy and psychology seriously, and had found it necessary to reconstruct them both in order to agree with the undeniable facts of his own varied and wide experiences. Recognizing James' originality and certain genuine contributions, Le Breton points out obstacles in the way of an objective evaluation and dangers of being misled by the persuasiveness of James' arguments and by the influence of his winning and resourceful personality. He credits James not only with a cosmopolitan view, but also with a keen sensitivity to, and a close contact with, the ups-and-downs of every-day life. He thinks that with the passing of years the specific contributions by James will become invisible but holds that his personal charm, seriousness of purpose, and great

penetration have given direction and vitality to philosophy. James' philosophy was so personal and unstable that if he had lived longer he would never have been able to put a final touch to it but would have continued to get new insights requiring reconstructions and reinterpretations. J. P.

A Comparative Study of Those Who Accept as against Those Who Reject Religious Authority. By THOMAS H. HOWELLS. University of Iowa Studies: Studies in Character, Vol. II, No. 2. Iowa City, The University of Iowa, 1928. Pp. 80.

The object of this experiment was to compare religious radicals and conservatives on the basis of a number of quantitative psychological tests. From 542 University of Iowa students in elementary psychology, the author selected, by means of a self-rating point scale for measuring intensity of beliefs and attitudes (reliability 0.92), 51 as the most radical and 50 as the most conservative. There were six times as many Catholics in the conservative group (23% of all in this group) as in the radical and the few Unitarians present were all in the radical group (3% of this group). Other denominations were more evenly represented in both groups. These extreme groups were given for comparison approximately 30 different tests—sensory-motor, volitional (suggestibility and perseverance), or intellectual in character, including a high school content examination and university grades. What the subjects reported about themselves in the self-rating was also taken into consideration. Considering these latter self-reported data first, we may note that the mean of the radical group was 18.9 years, and of the conservatives it was 19.7 years. In both groups women were in the majority, but the proportion was greater in the conservatives. Allowances for slight sex differences in the tests did not alter appreciably the results. The conservatives were as a group somewhat less healthy and inclined toward pessimism; they came from somewhat larger families and had had more religious training and sympathetic relationships with parents and also more mystical experiences (conversions, sense of divine presence, etc.). These group-differences, however, were small and probably not very significant. The tests revealed only a few sensori-motor differences approaching reliability, the conservatives being poorer in weight discrimination and muscle coördination, but slightly more sensitive to (expected) electric shock than the radical. This latter difference seems from the data, however, to be due to greater suggestibility clearly revealed by reliable differences on several tests. The conservatives excelled slightly in accuracy of slow arm movements, but were less deliberate and more inclined to hasty action. None of the sensori-motor tests revealed a group difference more than 3.7 times its PE. The conservatives accepted more suggestions as to printed (impersonal questions) relating to pictures recently shown them than did the radicals. For example, to the question "Did you see the man (not present) holding the oar?" They were more apt to say "yes." While the group difference here was only 3.1 its PE, the conservatives were reliably more ready to accept suggestions as to increase of pain from electric shock (when there was no increase in the current) and less persistent against painful stimulation. Threats of punishment effected greater improvement in them. They were also reliably inferior to the radicals in tracing mazes, in the high school content examination, and in the intelligence tests. The group differences in ability to follow written directions, to make moral judgments of the

multiple choice type (choosing the best and worst of four hypothetical responses in described situations), and in university grades all favored the radicals, but these differences were slightly below the point of statistical reliability.

It is certainly worth while to follow out more fully the causes of this intellectual and volitional inferiority of conservatives, and to determine the relative rôles of the lack of educational opportunities, the operation of various selective influences by religious groups for individuals of inferior ability and exclusion of superior individuals because of non-conformity, etc. The investigation indicates lines of approach to several important problems in social psychology and of inter-group and inter-national relations. There is a bibliography of 72 titles; and the self-rating tests, and additional laboratory test data are given in appendices.

J. P.

Abnormal Psychology: Its Concepts and Theories. By H. L. HOLLINGWORTH. New York, Ronald Press, 1930. Pp. 590.

This is in many ways a notable volume. In it Hollingworth discusses the facts of abnormal psychology as part of general psychological science, and not from any one partial standpoint, such as psychiatry, mental hygiene, clinical psychology, or psychoanalysis.

The book begins with a definition of the field of abnormal psychology and then turns, after a brief historical summary, to a consideration of the modern period in abnormal psychology. Among the contemporaneous viewpoints which are explained and evaluated are the anatomical, the physiological, and the psychological. A special chapter is also devoted to what the author ingeniously calls the *psychoanalogical* approach. This approach he says is "sometimes meaninglessly called the psychoanalytic."

The next general section of the book deals with the various classes of mental deviations from the normal. A brief summary of the material covered in this main body of the work may possibly best be given by listing certain of the main topics of the chapter. They are: feeblemindedness; origin of functional disorders; Janet's conception of psychoneuroses; typical psychoneurotic cases; the clinical views of Babinski, Hurst, Rosanoff, Freud, Adler, and Rivers; association and conditioning; objective study of the neurotic constitution; the psychology of the neurosis; stage-fright, dreams, stuttering, and aphasia; the psychological correlates of epilepsy; constitutional psychopathic states; personality types and the functional psychoses; and the relationship between drugs and mental disorders.

The fundamental theoretical concept of this volume may probably be said to consist in an interpretation of redintegration and integration. In developing the importance of redintegration the author points out that neurotics are as a group physically and psychologically inferior, and are affected by conditions which would not influence the ordinary individual. Typical in the history of each neurotic there appears a past environmentally determined episode from which the particular morbid symptom or symptoms under investigation date. There are, however, always discernible *present* stimuli, however reduced or attenuated, which redintegratively evoke the complaint, and in the absence of which the symptoms are abated and may, indeed, disappear. Particular pathological symptoms may therefore be changed by altering the environment or by changing the individual as

in re-education. Such modification either of environment or organism, however, does not necessarily change the individual's psychological inferiority which shows itself in a proneness to develop new episodes and thus generate new symptoms.

Many other important practical observations and suggestions are given in the volume, but the reviewer believes that its major contribution is the explicit application as described above of the conception of redintegration to an understanding of mental abnormality. Redintegration is often spoken of as if it were a theory or hypothesis, but, at least in the reviewer's opinion, when correctly understood, it is merely a statement of facts which are attested by unimpeachable experiment and almost universal observation. Hollingworth in previous works has possibly done more than any contemporaneous psychologist to systematize the implications of redintegration in so far as they apply to normal mental life. In the present volume this concept has with equal success been applied to abnormal mental life. In the reviewer's opinion this whole body of work, both on normal and abnormal psychology, presents a system which is of great importance to one who wishes, with a minimum of speculation, to unite the psychological facts of observation and experiment into a coherent and explanatory psychological theory.

Brown University LEONARD CARMICHAEL

General Psychology for Professional Students. By A. R. GILLILAND, JOHN B. MORGAN, and S. N. STEVENS. New York, D. C. Heath & Co., 1930. Pp. vii, 439.

We are told in the preface that "this book was written to fill the specific need for a special text for professional students." This alleged need will be met, the authors announce, "by a text that emphasizes no particular point of view, stresses objective material and practical illustrations rather than theoretical discussions and selects the illustrations from the fields of industry and the professions rather than from those of art, literature, and science."

After the introductory chapter on the nature and purpose of psychology, in which the assumptions and techniques of psychology are balanced by some pedagogic advice to the student as to his attitude toward the study of psychology, the book deals in a simple, lucid style with chapters on the human organism, innate behavior, sensory processes, attention, perception, and the usual order of topics in orthodox textbooks of general psychology. The final chapter on mental health is the only significant departure from the usual choice of topics for discussion.

Although no distinctive point of view is made explicit, there is nevertheless interwoven with the discussion of facts and illustrations an implicit interpretation of facts and observations in terms of action psychology. Perception is defined as the "result of actual responses to stimuli." Memory is treated as "a form of the learning process in which the verbal components are more significant than are other types of response." The essentials of thinking are discriminating and generalizing which are described as "acts in response to a maladjusted situation." Insight, we are told, results from experimenting with a new situation in the light of previously experienced situations. Finally, it is held that the best definition of intelligence is the one that characterizes it as "the ability to make successful adjustments to the problems of life."

The text is amply illustrated and many of the figures and illustrations are original. To each chapter is appended a selected list of references representing every significant point of view in psychology. The title seems to be a misnomer. Neither the choice of material nor the method of treatment is distinctive enough to warrant the claim of a special type of general psychology.

University of Montana — FRANKLIN O. SMITH

Chinin in der Allgemeinpraxis unter Berucksichtigung pharmakologischer Befunde. By FRITZ JOHANNESSOHN. Amsterdam, Bureau tot Bevorderung van het Kinine-Gebruik, 1930. Pp. 232.

This book is essentially a review of 652 articles or works which throw light upon the problem of the physiological and therapeutic effects of quinine (Chinin), and of 157 articles which have to do with the effects and applications of "Chinidin," a less completely extracted form of quinine. All of the articles reviewed have been published since the close of the World War.

The opening sections of the book deal with the topic of the occurrence of quinine. The greatest part, however, deals with the physiological and therapeutic effects of the drug. The book will, therefore, claim the attention of the medical man to a much greater extent than that of the psychologist.

The author has rendered an important service in giving a clear and concise summary of the widely scattered literature upon quinine. The work is perhaps defective as regards the touch of originality; the author's name appears once only in the two long bibliographies. The material is well organized and the style is direct.

Wellesley College — MICHAEL J. ZIGLER

Psychology for Students of Education. By ARTHUR I. GATES. Revised Ed. New York, Macmillan Co., 1930. Pp. xv, 612.

The revisions fall under two general heads: those which are made for the purpose of presenting material which has a maximum value in the work of the teacher, and those made to bring the text in line with the developments made in the field of psychology since 1922. One change of the first type has to do with the problems of physiological psychology. In the old text three chapters are devoted to such problems while in the revision this material has been reduced to one. The omitted material is concerned almost entirely with sense organs and localization of functions. The omission of the chapter on conscious states and processes is also to be noted. The most fundamental revision due to new developments is in the treatment of instincts. In the new text emphasis is placed on the theoretical aspects of the problem, along with the implications for education. A considerable amount of this material is put under the heading of motivation. Greater emphasis is now placed on personality and personality adjustments, and on researches in growth. Gestalt psychology, emotions and feelings in the life of the child with consideration of some individual cases, and more specific material on the general laws of learning—particularly from the field of psychology of reading and of other school subjects—might well have received more space and attention; and the chapter on the nervous system could have been made more up to date and useful. Our teachers of psychology who use the book have supplemented and modified the materials presented as seemed necessary.

University of Texas — C. T. GRAY

The Behavior of the Newborn Infant. Ohio State University Studies, Graduate School Series: Contributions to Psychology. No. 10. By KARL C. PRATT, AMALIE KRAUSHARR NELSON, and KUO HUA SAN. Columbus, Ohio State University Press, 1930. Pp. xiii, 327.

This investigation was carried out with commendable objectivity under the direction of the late A. P. Weiss and Andrews Rogers (1) to determine the reactions of infants to certain stimuli at birth and the development of these reactions with age, and (2) to note the appearance of 'new reactions.' The investigators carried out their studies on from 30 to 70 infants beginning one to four hours after birth and continuing for about two weeks. Every infant was put into a specially constructed experimental cabinet where temperature, light, sound, etc., could be rather accurately controlled. Provision was made for the automatic recording of responses in the head-foot and the right-left direction of each infant to both general and specific stimuli during the daily observation period, and two special windows were provided through which the experimenter and the recorder could make observations and records in order to get more additional data than either of these could record by himself and also to serve as special checks on each other's accuracy. By means of easily-marked symbols devised to designate different, carefully classified objective responses, the most commonly observed specific movements, postures, and conditions of the infants were recorded while the latter were in the cabinet. The experimental session was divided into 'control periods' and an 'experimental period.' The former were periods of no specially applied stimuli, intended to serve as a check on responses obtained in the experimental periods. During any 'control period' the recorder observed the infant through the small window at his end of the cabinet, noting down on the tape of the Renshaw polygraph various data as to the time of day, hygrometer readings, position and condition of the infant, etc. The polygraph was then started and a paper strip three and one-half inches wide was unreeled. Upon this the activity pens and the time-marker inscribed permanent records of the application of the various stimuli and the resulting movements as indicated above. Upon this strip the recorder also noted by symbols certain other specific movements of the infant. Thus both automatic records and observed records were made in their true time relation. During the experimental periods stimuli of different kinds were carefully applied, and reactions to light, sound, taste, smell, temperature, and to the holding of the nose and also of the arms were recorded as already described, the time and duration of each stimulus showing on the time line.

The methods used were admittedly somewhat crude and mechanical and of a preliminary nature. The authors find that "the infant is not a man in miniature." It is equipped soon after birth "with quite a number of reflexes, but the degree of their specificity and their significance seem to have been unduly exaggerated" (p. 208)—a natural consequence, one may infer, of the earlier merely observational methods. The child sneezes, coughs, hiccoughs, cries, etc., and does in a general way adjust itself to its environment. Movements which have to do with swallowing are fairly well coördinated: those movements which have to do with getting food, such as "piercing cries, high and almost convulsive strampling activity, while not coördinated in the muscular sense, do act as strong stimuli on the social environment so that something is done about it," *e.g.* the child is fed. Similar reactions are given to such external conditions as decreasing temperature. "It seems certain

that such well coördinated reactions as those implied by the terms love, rage, and fear do not exist at this early age" (p. 211). The authors found that the double-check method of observation was valuable. The reactions associated with specific stimuli which also occurred during the non-stimulus periods could certainly not be attributed to such stimuli although many qualitative observers have so attributed them. In the responses to external stimuli, the infant's behavior is generalized so that a stimulus affecting any group of receptors will produce a response in almost any part of the organism; but the reactions tend to manifest themselves most strongly in that part of the organism which is stimulated, and from there it spreads out in decreasing intensity to other segments of the body. The authors say that "even within the first two weeks one may observe a restriction of the generalized reactions as indicated by a decrease in the number of effector segments involved and an increasing specificity in their reactions to stimuli. We are not in a position to ascribe this increasing specificity of function as primarily due to either maturity or environmental factors" (p. 208). It is obvious that earlier observations cannot be relied upon in the light of these results by more accurate and objective methods. J. P.

Intellectual Growth in Young Children. By SUSAN ISAACS. With an appendix on children's 'Why' questions by Nathan Isaacs. New York, Harcourt, Brace & Co., 1930. Pp. xi, 370.

This is the first of a series of three volumes on child development. The data are based upon verbatim records of the activities and conversation of a small group of children in a private school at Cambridge, England, during a three-year period. All the children were from the professional classes, "many of highly distinguished parents." The average mental ratio on the Burt revision of the Binet scale was 1.31. The superior endowment of the subjects and their stimulating intellectual environment must be kept in mind in interpreting the findings.

Isaacs points out that the verbal responses of children to a series of standardized questions and exercises in a mental test given by an adult do not always afford a true picture of their actual ability to make rational judgments or to plan resourcefully when motivated by the needs of a concrete situation. She takes issue with Piaget's view that interest in mechanical causality and ability to appreciate it do not normally develop until the middle years of childhood, and cities a number of spontaneous comments and questions put by children of 3-5 yr. of age which appear to show a level of understanding far in advance of that reported by Piaget, whose conditions of observation and experiment were not, in her opinion, favorable to the manifestation of logical thinking on the part of the child. As a result of her observations she comes to the conclusion (p. 84) that "the difference between the younger child and the older, between the child and the adult, is thus *not* that the former do not reason or reason *only* in the form of the perceptual judgment and practical manipulation. It is rather the extent to which, with the younger children, the higher forms of noetic synthesis rest directly upon and grow immediately out of the simpler. Verbal reasoning and the clear formulation of judgments are no more than wavecrests upon the flow of young children's thought. They spring up from a practical or personal situation, and die down again to it. Verbal thinking can hardly yet be *sustained in its own*

right, in the earlier years. It draws its vitality from the actual problems of concrete understanding and manipulation in which it takes its rise, and the solution of which it furthers. One of the marks of growth is thus the increasing independence of cumulative thought from immediate concrete and personal situations."

The major portion of the book is given up to accounts of individual incidents and conversations among the children illustrating the author's point of view. These reports are often highly suggestive, but the failure to arrange the material by age-levels or to group the data for the separate children in an orderly sequence makes it difficult to interpret the findings in terms of the growth trends suggested by the title. The treatment throughout is descriptive and philosophical rather than experimental.

University of Minnesota FLORENCE L. GOODENOUGH

The Social and Emotional Development of the Pre-School Child. By K. M. B. BRIDGES. London, Kegan Paul, Trench, Trubner and Co., Ltd., 1931. Pp. x, 277.

The greater part of this book consists of a detailed description of two scales, one for scoring the social development, and the other the emotional development, of the pre-school child, as they were formulated and applied to a group of 28 children in the McGill University Nursery School. The author observed the children for a period of a month, with respect to the items to be scored, before the actual scores were made on the basis of notes taken during the month. Much of the subjectivity of the usual rating scale was avoided by the method of scoring the occurrence of specific types of behavior under given situations. For example, a child was given a point on the social development scale if he had "sought another child's approval" or "comforted another in distress." The careful description of the types of behavior considered under each item, makes possible duplication by others of the method of scoring, and, in addition, gives an accurate picture of the behavior of pre-school children.

The Social Development scale is made up of two sections—(I) Relations with Children; and (II) Relations with Adults,—each of which is subdivided into socially desirable and undesirable forms of behavior. These are so worded that a point score is always given to the desirable reaction, and the final score is the percentage of observed responses which are desirable.

The scores obtained by the nursery school children show that, although there are individual variations, "children between the ages of two and five years pass through" first a "dependent stage" where "the child is somewhat passive and relies on the adult for assistance and attention"; secondly, between two and a half and three years, a stage of "resistance against adult influence and striving for power and independence." This attitude gradually changes to one of coöperation and friendliness, after the fourth year.

The Emotional Development scale, which is scored in the same way as the other, is divided into six sections—(I) Distress and Tears, (II) Fear and Caution, (III) Anger and Annoyance, (IV) Delight and Affection, (V) Excitement and Enuresis, and (VI) Mannerisms and Speech Anomalies. The scores on this scale showed very slight age differences. Far more noticeable was the increase of socially desirable emotional behavior during the first few months of nursery school attendance—evidence of the importance of experience in determining

emotional behavior "and therefore the emotions." Wide individual variations were found, regardless of age. The kinds of stimuli which elicit the emotional behavior indicated under the section headings, are discussed.

A chapter on A Genetic Theory of the Emotions describes the increasing variability of emotional behavior in the growing child, from the undifferentiated excitement of the new-born infant, to some 15 readily discriminable forms of emotion in the 5-yr.-old. There are additional chapters on a character rating scale, and comparisons of the scores of individual children on the three scales. A large number of photographs of the children in the nursery school illustrate characteristic responses on which the scores were based. There is a bibliography and an index.

University of California NANCY BAYLEY

An Experimental Study of the Iowa Placement Examinations. By LAWRENCE WILLIAM MILLER. University of Iowa Studies, Studies in Education, Vol. 5, No. 6. Iowa City, University of Iowa Press, 1930. Pp. 116.

On the basis of careful analyses of new and extensive data and of weightings by multiple correlation new materials were devised and tested with a view of improving the placement examinations.

Duke University W. A. BROWNELL

Biblical Information in Relation to Character and Conduct. By PLEASANT ROSCOE HIGHTOWER. University of Iowa Studies, Studies in Character, Vol. 3, No. 2. Iowa City, University of Iowa Press, 1930. Pp. 72.

Hightower attempts in this monograph to learn whether mere knowledge of the Bible affects the development of character in children. He wishes specifically to discover how the child's conduct is affected when exposed to the temptation to cheat.

The methods of investigation conformed to accepted standards of research. The results indicate no significant relationship between Biblical information and conduct. The author concludes, therefore, that the methods of instruction and the "curricular arrangement of Biblical material" must be altered if desired results are obtained.

Newcomb College, Tulane University DAGNY SUNNE

La science du caractère. By W. BOVEN. Neuchatel, Delachaux et Niestlé, 1931. Pp. 345.

This work, an *essai de caractérologie générale*, is an instructive sample of a type of psychological writing common in Europe, but not represented in America. The author draws freely from both the German and the French tradition in characterology. His method, however, resembles less the dialectical attacks of Utitz, Künkel, Ziehen, and Hoffman than the pure schematization of Paulhan, Malapert, Achille-Delmas, and Boll.

According to Boven the structure of character consists basically of three systems, the vegetative, the motor, and the sensorial, through which operate seven

major dispositions (desire, domination, pain, pleasure, fear, anger, and sympathy) that constitute the first *assise* of character The second *assise* comprises the level of traits which are "attitudes habitually adopted" toward the self, others, things, and abstractions. The third *assise* is composed of *linéaments* representing higher integrations or "prolongations" of traits into values, intentions, or concepts. The higher levels are more educable than the lower.

This doctrine of the constitution of character is overlaid and obscured by the bulk of the book which presents a spacious view of certain general topics. The author hopefully undertakes to illuminate the field of character through a survey of heredity, embryology, intelligence, race, sex, age, disease, and education. The reader is likely to feel that the treatment is indistinct, and that the scientific study of personality demands a sharper focus.

Harvard University GORDON W. ALLPORT

Legal Psychology. By H. E. BURTT. New York, Prentice-Hall, 1931. Pp. xiv, 467.

The author wants to show lawyers what possibilities there are at the present time for applying the modern technique of psychological measurement to problems which may occur in the practice of law. After a somewhat popularizing introduction, a long chapter (42 pages) summarizes the most common forms of illusions of sensation and perception. Two briefer chapters then point out the difficulties most commonly obstructing attention and memory. A further chapter discusses hypnotism from its mildest aspect in "the leading question" to the probabilities of post-hypnotic influences. The value of testimony and confession and problems of the jury are then taken up. The detector methods of word association, breath recording, and pulse recording are treated in three chapters, together covering 80 pages. Two chapters of 74 pages are then given to the mentally disordered and the mentally defective criminal. The possibility of discovering in childhood and early youth symptoms of later criminal tendencies is then discussed in a brief chapter, somewhat too optimistically as it appears to the reviewer; but the author could hardly take here a pessimistic attitude.

The reviewer does not understand how the next chapter, the one on eugenics (31 pages), came to be fitted into a book on legal psychology, as it has no relation to the practice of law.

The chapter on punishment, which follows the one on eugenics, reiterates the arguments in favor of the possibility of making mild punishments, through certainty of application, as deterrent as severe, but rather uncertain, punishments. In a chapter on drugs the author joins those psychologists who from experiments on extremely simple reflexes conclude that the belief of having taken a drug might be equivalent to an actual drug effect. The reviewer is peeved because the author is unacquainted with his method of studying the effects of drugs on highly intellectual processes, which seemed to show that the expectation of a drug effect does not produce a result similar to that of a drug.

Two chapters on crime suggestion and crime prevention, being purely sociological, do not seem to fit in the book any more than that on eugenics. The last chapter on trade-mark infringement points out to the legal profession that "the

degree of confusion" can be measured more exactly than various court decisions seem to suppose, but admits that "many other things enter into a decision."

There was a need for a book like this and the author deserves the gratitude, not only of the lawyer, but also of the psychologist, for having gathered whatever seemed to him to belong together, although opinions may differ slightly in this respect.

University of Missouri MAX F. MEYER

Psychology for the Lawyer. By D. G. MCCARTY. New York. Prentice-Hall, 1929. Pp. xiii, 723.

If the matter directly quoted were to be removed from this book, it would cover a little more than 300 pages. That, however, is no fault for the lawyer, since lawyers are accustomed to books made up of quotations. In reviewing the book for psychologists, the psychologist can hardly abstain from expressing the judgment that it is of very mixed quality. There is at least one chapter which to the scientific thinker is trash. There are other chapters, and not a few, which the psychologist will read with high appreciation of the author's keenness in analyzing problems of the court room and of legal practice. Such are the chapters on Bias, Intention in the Law, Psychology and the Law of Evidence, Responsibility and the Criminal Law, and Knowing the Client.

The introductory chapters, which give the lawyer an exposition of general psychology, are full of contradictions. For this the psychologists whom he has read are perhaps more to be blamed than the author. He has taken the quibbling of the 'schools' too seriously, as most psychological laymen are apt to do. But how a whole chapter on "phrenology with the bumps moved on the face" (so the author, quoting Allport, contradicts himself) could have gotten into the book, is difficult to understand. The chapter on Strategy in a Lawsuit is extremely interesting because it shows that a successful lawyer's ethics must by necessity be of the same quality as that of the successful warrior; that is, brutal and unscrupulous. The professional ethics of the practice of law has about the same meaning as that which the international law has in actual warfare. The psychologist who is looking for problems in social psychology to the solution of which he might apply some of the methods which social psychology is developing, will find in the book a rich source of suggestions, sometimes contrary to the author's intentions, but often set forth with clear intention, especially in the chapters which the reviewer has above enumerated as the best of the book.

University of Missouri MAX F. MEYER

Psychology in Modern Business. By HARRY WALKER HEPNER. New York City, Prentice-Hall, 1030. Pp. xv, 728.

This is in many respects a useful book in spite of the fact that it falls into the error of most books on applied psychology; namely, it covers too much ground on the psychological side and, on the side of application it includes too much from fields other than psychology. The first 150 pages of the book are to give the business man insight into the "Nature of the Mental Life." But the reviewer doubts the possibility of realizing such an ambitious scheme. The author wants to protect the business man against quack methods. But can

that be done by dubitable bits of information like the following? "To understand why people behave . . . it is necessary for the business man to have an acquaintance with glands." "Hysteria is more common among . . . low . . . than . . . high intelligence." In the opinion of the reviewer nothing would have been lost if the book had started on page 151 with "the pseudo-scientific methods of analyzing people." One could apply, with adaptation, to the first part of the book what the author says on page 535: "We attend the theater (the business man reads these pages), not to see life (psychology) as it is, but to see it in a romantic situation." More or less this applies to the whole book. It is a readable book, a good seller.

The bulk of the book is divided into four parts, the second and third having the individual in view, the fourth and fifth the group; but it is clear that the distinction is more the result of book-makers practice in pleasing the reader by a conspicuous classification than the result of psychological science. So we find Credit, Hiring, Vocational Choice, and Salesmanship, treated as individual problems, but reappearing as group problems with little change of "psychology" under the titles of Good Will, Social Services, and Advertising Devices. But this statement is meant only as a criticism of the scientific value to the psychologist, not as a criticism of the practical usefulness of the book for the business man. The material has been largely collected from trade publications and commercial magazines.

The professional psychologist who is not a business technician may be most interested in the last sixty pages, the sixth part of the book, which gives rules for undertaking any business psychology research. The limitations of the laboratory method are pointed out. The questionary is critically discussed. A very brief account is given of the statistical treatment of rankings. A few interesting tests are printed. The meagerness of the sixth part compared with the four parts preceding it suggests the question: How far has applied psychology advanced beyond being a mere study by the descriptive "case method"? The author (p. 549) is right: "Abstractions do not impress the reader." Therefore he has reduced them to a minimum.

University of Missouri MAX F. MEYER

The Remaking of Marriage: A Contribution to the Psychology of Sex Relationship. By POUL BJERRE. Translated by T. H. Winlow. New York, Macmillan Co., 1931. Pp. viii, 257.

Two very brief quotations indicate the nature of this work. "In the series of chapters beginning with Love and Anxiety and closing with Heroism and Tragedy I present various views and join them loosely together without attempting a systematization" (p. 11). "First of all, as far as the psychological standpoint is concerned, we reject all systems as human inventions and proceed directly to nature with that will to lucidity which has been given to the mind generally, and to the subconscious especially" (p. 19). The book is more of the nature of the memoirs of a practicing psychiatrist than it is a scientific treatise. The author believes the chief functions of sexual relationships are to ward off loneliness, to smooth out life's difficulties, and to secure a 'renewal of the spirit.' Marriage is an attempt to find one's self, and loneliness is the only valid cause

for divorce. He recommends (p. 216) a period of betrothal which is equivalent to legalized free love, but suggests that legitimization should not include publication. On the other hand, he considers best of all what he terms "the sacramental union," a joint struggle through all life's difficulties. This, apparently, is to be entered upon if the experiences of the betrothal period justify it.

Southwestern University W. R. ATKINSON

The Influence of Training on Changes in Variability in Achievement. By HOMER B. REED. Psychological Monographs, No. 185. Princeton, The Psychological Review Co., 1931. Pp. 59.

Reed gives a critical evaluation of eight different statistical techniques that have been used in studying changes in group variability with training. He selects three of these, viz., (1) coefficient of variability, (2) correlation between initial score and percent gain, and (3) ratio of best to worst before practice compared with ratio of same individuals after practice, as being best adapted to the problem. These three measures are reported for the data of 25 published investigations, Reed computing the measures himself whenever not given by the authors. It is pointed out that when the same measures are used, the apparent discrepancy between the results of different investigators disappears, the great majority of results showing a decrease in variability with equal training.

A statement frequently repeated by Reed throughout the monograph is that the S.D. takes into account only the middle two-thirds of the cases. This seems to be based on a misinterpretation of the fact that a distance of $\pm$ 1 S.D. from the mean will, in a normal distribution, include roughly two-thirds (actually 68%) of the total number of cases. This does not imply that only those cases are taken into account by the S.D.; the extreme deviations have, in fact, more weight than those near the mean in determining the size of S.D., since each deviation is squared.

In addition to his analysis of published data, which takes up the larger part of the monograph, Reed reports two new researches on the problem. The first is on practice in a motor task, viz., throwing the yo-yo top. The subjects were 37 college sophomores. They had a total of eighteen 10-min. practice periods, the score being the number of successful 'catches' during each period. In the second study, the Stanford Achievement Test and the Compass Survey Arithmetic Tests were given in October, February, and May to 137 school children in grades 2 to 8 inclusive. The 'practice' consisted only of the ordinary classroom work intervening between the three testings. The various measures of variability computed in the two studies again show a decrease in individual differences after training. In addition, the probability of a decrease in variability seems to be greater for the extremes of the distribution than for the individuals closer to the mean. Reed writes about these results that "the greater the difference between individuals in achievement is the greater the probability that training will reduce this difference." (p. 53). His data suggest only that this is true *within* any one distribution; no evidence is given that it applies to comparisons *between* distributions. Finally, Reed suggests the possibility that the amount of reduction in variability may be a function of the amount of gain during the training period. His evidence is that in the study on school-children, greater gain as well as greater reduction in variability occurred from October to February

than from February to May. This raises an interesting problem which should be investigated, and we wonder why similar results are not reported for the data on the yo-yo top. In that study, practice was more carefully controlled than in the study on school children, and furthermore, there were 18 rather than 3 records on each subject. Such data would have been much better adapted than those used to studying the course of decrease in variability and its relation to the course of improvement in performance.

The criticisms suggested above do not in any way detract from the fundamental contributions of Reed's study, which seem to be, first, the demonstration of the essential agreement among published results on the effect of practice on variability, and secondly, the suggestion of a number of questions which require further investigation.

Columbia University ANNE ANASTASI

Das Genie-Problem. By W. LANGE-EICHBAUM. Munich, Ernst Reinhardt, 1931. Pp. 128.

This little book is nineteenth century in spirit, in method, and in such few allusions as are made to the work of scholars other than the author. Gravely assuring his readers that an analytical study of genius should no more destroy its enchantment than should the study of botany and astronomy detract from the beauties of nature, the author proceeds to outline his field, and succeeds in making it appear very complicated indeed. The necessary and sufficient condition of genius are discussed in Chapter 1. In succeeding chapters are considered (1) sociological aspects—fame and milieu; (2) endowment of mind and body; (3) genius and insanity. Problems (1) and (2) are handled as meticulous expansions of the definition of genius formulated in Chapter 1: "a bearer of spiritual values who is revered by many." The bearer, the values found in his accomplishment, the recognition accorded him, and his admiring public, are methodically discussed under such headings as "disposition and aptitude;" "inborn talent;" and "the origin of fame."

The section on problem (3), genius and insanity, is not closely related to the main discourse, but deals with the incidence of psychoses and psychotic traits among men of genius, and seeks to establish an intimate functional relationship between genius and mental disorders. In this field, where vigor of thought and positiveness of statement cannot substitute for research data scientifically collected and interpreted, the author is most inadequate. Disregarding all modern studies of the much discussed subject, he builds up from within a remarkable series of statements and inferences. Of these, the climax is the conclusion that while genius and madness may not be synonymous, the insane are more liable than others to become eminent!

Pasadena City Schools BARBARA S. BURKS

Animal Motivation: Experimental Studies on the Albino Rat. By C. J. WARDEN, with the Collaboration of T. N. Jenkins, L. H. Warner, Marion Jenkins, E. L. Hamilton, and H. W. Nissen. New York, Columbia University Press, 1931. Pp. xii, 502.

This volume brings together 13 reprinted papers and one new article reporting researches on animal drives. The entire project is an attempt to measure and

compare the maximal strengths of the maternal, thirst, hunger, sex, and exploratory drives under definitely standardized conditions. The reviewer feels that it is probably the best work that has been done in this field. It is superior to previous studies in its methods and apparatus, which were definitely standardized after much preliminary experimentation, in its detailed statistical analysis of the results, and in the intrinsic value of the results and conclusions themselves.

The method used in all the studies, except the four in the appendices, is the well-known obstruction method which involves the placing of an obstruction (electrical grill) between the animal and the incentive. Indices of the relative strengths of the drives are in terms of the number of times the animal passes from the entrance compartment through the obstruction compartment, into the incentive compartment in a certain period of time (here 20 min.).

Since all of the papers making up the present volume, except the one which compares the maximal strengths of the drives, have appeared elsewhere, we shall deal principally with the latter.

The different drives are considered to be maximal after deprivation periods of one and two days for the females and males, respectively, for thirst, and two and four days for hunger. Sex deprivation for one day for the males and the female in oestrum produce the peaks of sex activity. For the maternal drive females who had given birth to their litters 15 hours before testing were used. For the exploratory drive no special arrangements were made to control the little-known intra-organic conditions influencing the tendency. There is, of course, no assurance that the last two drives were maximal. The incentive used for the maternal drive was the living cage containing the litter of young. This means that the maternal drive plus the tendency to return to the home nest were both involved. This possibly accounts in part for the great motivation under these conditions. The authors recognize these limitations. Under these conditions the drives are ranked in strength as follows: maternal, thirst, hunger, sex, exploratory.

Besides these limitations, there is another objection: when comparisons between the drives are made on the basis of 5-min. periods instead on that of total scores, significant differences in persistence are found. This means that the ranking of the drives is in part a function of the length of the test period. If the drives are ranked on the basis of every 5-min. period, thirst decreases in rank from first in the first two periods to fourth in the last. On the basis of totals it is placed second. The exploratory drive is the only one that maintains a constant rank throughout.

Since we are comparing the maximal conditions of the different drives perhaps we should use the period showing the *greatest* number of crossings. These we find pp. 385, 393, to be thirst in the first 5-min. period (153 times), maternal in the first (126), hunger in the third (100), sex in the fourth (94 both sexes), and exploratory in the first (48). These comparisons lessen somewhat the significance of the final rankings given by the authors. If the test period were 10 instead of 20 min. in length, the peak of activity would be after a deprivation period of two instead of three days for hunger. Table 5 (p. 205) shows that the 35-days-segregated males, after 28 days of sex deprivation, cross the electric grill to get to a cornified female 16, 40, 54, and 101 times, respectively, during each of the four successive 5-min. intervals. After only one day of sex deprivation they

cross 76, 63, 60, and 70 times during the corresponding periods. The totals are 211 and 269, respectively, and the authors conclude that "after one day [of deprivation] the sex drive seems to diminish slowly in strength as the segregation period lengthens" (p. 212). The results suggest that if a longer text period had been used a somewhat different conclusion would be justified. And, after all, a 20-min. test period is rather arbitrary! The authors admit that the meaning of the "loose concept of persistence of drive is not at all clear" (p. 384), but their failure to take it into consideration lessens somewhat the significance of some of their comparisons.

A few errors occur. The "22" in the column at the extreme right of Table 3, p. 385, opposite "Sex-female, Oestrum," should be "122." On pages 78 and 80 a one-day starvation group is mentioned, but so far as the reviewer is able to discover there is no such group in the study. There is much unnecessary repetition in the details of apparatus and procedure, and in the bibliographies. Every oneof the seven parts and the separate appendices has an introduction by the senior author.

University of North Dakota C. W. TELFORD

Child Psychology. By JOHN J. B. MORGAN. New York, Richard R. Smith, Inc., 1930. Pp. viii, 474.

After an introductory chapter on Methods and Objectives of Child Study, the author lays the foundations for his later discussion in a chapter on Infant Behavior. In this part, he makes effective use of recent experimental data gathered from studies on infants, in order to show the genesis of later attitudes and more fully developed behavior. The subsequent chapters are written chiefly from the point of view of the clinical psychologist. All in all, the reader may get from them a fair notion of the possibilities of education and control, as well as the best means by which desired ends may be accomplished. Advice for guidance and for treatment of problem children is concretely given with examples drawn from the author's experience. A list of the additional chapter headings will indicate the subjects covered. The Child's Adjustment to His Family, Motor Development, Emotional Development, Motivation, Development of Meanings, Imagination, Language, Verbal Learning, Thinking and Working, Play, Social Development, Intelligence, and The Integrated Child.

In the preface, the author expresses the belief that the book will be of value to teachers and to parents, and that it may serve as a text in child psychology. For the latter purpose, it seems to the reviewer somewhat elementary, although on the whole, it may be recommended to such of the former as do not possess any special knowledge of the subject, and who will not be too much annoyed by the trivial platitudes which so frequently fill up the spaces between useful facts and wise clinical insights.

University of California NANCY BAYLEY

Cope: Master Naturalist. By HENRY FAIRFIELD OSBORN and others. Princeton, Princeton University Press, 1931. Pp. 740.

This is a life story of a great naturalist and it is well worth the perusal of all students of natural science. Many of Cope's letters are reproduced which not only make pleasant reading but which throw important sidelights on his scientific ideas.

Cope was superlatively able and productive in comparative anatomy and vertebrate faunal zoölogy and paleontology. His personal experiences and human contacts, his travels and dealings with all sorts of people, his general reading and philosophical speculations, his interest in comparative ethnology, in human and anthropoid variability, in phylogeny and evolution, in archeology and pre-history, and his special work in osteology, craniology, and comparative odontology connect him with the anthropological and psychological sciences.

A classified bibliography of 1395 titles lists a number of interest to the psychologist—in particular the genetic observations and discussions on the habits of many types of animals.

New York City WILLIAM HARPER DAVIS

Social Psychology. By JOSEPH K. FOLSOM. New York, Harpers, 1931. Pp. xviii, 701.

The expressed wish of the author is that his book will prove equally acceptable to psychologists, sociologists, and cultural anthropologists. Basic to this hope is the treatment of social psychology from three different angles; namely, individual behavior approach, social interaction approach, and cultural approach. These three approaches to the field of social psychology are treated in the order indicated and, if the author's judgment is our criterion, they have been integrated into a well organized whole. Before questioning the validity of this judgment, it should be admitted that Folsom has drawn intensively upon many inductive studies of social behavior, although his claim to have covered all of the important discoveries and conclusions of research in social psychology need not be taken seriously.

Undoubtedly sociologists and cultural anthropologists are capable of evaluating Folsom's contribution to their respective fields. In the reviewer's opinion, psychologists will easily detect the absence of integration within the "approach trinity" and, in addition, they will question the pertinence to social psychology of the greater part of the material arrayed under the individual behavior approach. Lengthy descriptions of the nervous system and discussions of theories of learning, occupying as they do approximately one-sixth of the total text, immediately confront the student of social psychology; yet the author kindly informs the persistent reader, "we are now to take the elementary processes for granted and to study the higher forms of organization" (p. 135). In similar manner chapters V and VI, entitled Wish Frustration and Personality Readjustment and Individual Personality Differences and Their Measurement (pp. 167-295), which conclude the individual approach, are not utilized appreciably in succeeding discussions. These two chapters contain excellent material for students of abnormal psychology and of personality measurement, but a reader ignorant of the book title could not be expected to guess it correctly from the contents of these chapters. The section of the text presumably of interest to psychologists is not an organized unit but a series of discrete topics. Had these been made to yield service to the two remaining approaches the book would command the interest of psychologists, but the lack of integration forces attention primarily to a few meritorious chapters.

University of Minnesota CHARLES BIRD

Academic Progress in the University. By HAROLD A. EDGERTON. Baltimore, Warwick & York, 1930. Pp. 83.

"This study has as its purpose the investigation of some of the possibilities of a system of cumulative academic prognosis." The sample consists of those students who entered the five largest colleges of the Ohio State University in 1924 and 1925. The criterion of academic success is the point-hour ratio during the student's first six semesters in the University (points being determined as follows: each hour of A, 4 points; B, 3 points; C, 2 points; D, 1 point; E, 0 points). Results are presented separately for each sex and for each of the five colleges. Briefly, it is found that the correlation between academic success and percentile score in the Ohio State University Psychological Examination (Intelligence Test) is, for the 1924 group, 0.517; for the 1925 group, 0.490. Between academic success, and a combination of intelligence test and point-hour ratio at the end of the *first* semester, the multiple correlations are 0.882 and 0.868 for the 1924 and the 1925 groups respectively; between academic success and a combination of intelligence test and point-hour ratio at the end of the *fifth* semester, the correlations are 0.979 and 0.983. Intelligence-test score contributes practically nothing to these last four correlations which is not already included in the point-hour ratio. It is concluded that academic success (*i.e.* point-hour ratio at the end of the first six semesters of work) can be reliably predicted from previous academic performance.

Edgerton's monograph supplies a careful, thoroughly competent study of an important problem. The statistical technique, in particular, merits commendation, among other things, for the use of Fisher's z—function of r. We are less certain as to the theoretical validity of the use of percentile scores in Pearsonian correlation. The defense to this, doubtlessly, is the practical fact that percentile scores are what the Ohio State University actually employs in its personnel work. The reviewer fails to understand why no attempt was made to determine, by some split-half technique, the reliability of the point-hour ratio; the fact that *ratios* are involved is not the statistical *kobold* that Edgerton (p. 17) would appear to believe.

The weakest point of the monograph doubtless lies in its serious omission. What is the correlation between academic performance in the first three semesters and in the *next* three? An answer to this question would constitute real prediction. Edgerton, *per contra*, presents only the correlation between performance in a certain number of semesters, and performance in a longer period which *includes all the past semesters but one.* This is not genuine prediction at all, even if it be "prognosis."

University of California HERBERT S. CONRAD

Mental Development of the Child: A Summary of Modern Psychological Theory. By KARL BÜHLER. Translated from the 5th German ed. by Oscar Oeser. New York, Harcourt Brace & Co.; London, Kegan Paul, Trench, Trubner & Co., 1930. Pp. xi, 170.

This volume is chiefly a summary and discussion of the early German contributions to the field of child psychology supplemented by considerations of the experiments of Köhler and those of C. Bühler, Hetzer, and others in the Vienna

laboratories. The plan of organization does not seem to the reviewer to be entirely consistent. There is an excellent and brief summary of comparative psychology in so far as it contributes to the study of the young child, and the treatment of physical development is clear and concise and has excellent illustrations. The author distinguishes the three factors operating in successive stages of behavior—instinct, training, and intellect. Instincts are considered to be "a ready-for-use inheritance of modes of behavior set going in a definite way according to a preformed natural plan." Training works by suppressing some, accentuating others and forming new combinations of the instinctive modes. Intellect is defined as "the ability to discover and invent." It is used almost synonomously with the Gestalt "insight."

The later chapters are highly theoretical and are chiefly expressions of opinion. There is little evidence that the author is familiar with the outstanding experimental studies by French and American investigators in recent years. His chapter on children's drawings does not include a mention of the Goodenough "Drawing Test" nor does his chapter on language reveal any knowledge of the work of Descoeudres, Piaget, Smith, and many others of importance. However, his approach to the development of concepts, judgments and reasoning in the child is strikingly similar in many respects to Piaget's. Although all the books of Piaget are listed as references at the end of the chapter on the Origin of Concepts, there is not a single mention of these works in the text. Inasmuch as this volume is a summary of the author's *Die geistige Entwicklung des Kindes* it probably should not be censured for incompleteness. However, any adequate "summary of *modern* psychological theory" (italics mine) should include at least some comments upon the outstanding recent experimental studies in the field. The revision thus leaves much to be desired. The first edition was published in 1919.

University of Georgia DOROTHEA MCCARTHY

A Syllabus for the First Course in Experimental Psychology. By S. L. CRAWLEY. New York, Henry Holt & Co., 1930. Pp. 177.

This is a practicable manual for use in schools with no or very limited laboratory facilities. Most of the experiments, of which there are 24, are of a type that makes possible their use with an entire class as subjects and the instructor as experimenter. The directions are complete, although sufficient provision is made for adaptation of technique to local conditions. The experiments outlined are of the usual sort, dealing with individual differences, observation and report, suggestion, sensory acuity, feelings, intelligence, learning, memory, etc. A bibliography of from 10 to 41 references for each experiment, consisting chiefly of citations in elementary texts, is quite complete for its type. It could have been improved, however, had certain of the best treatments been starred, or those books indicated which give practically the same summary of any one topic. Detachable sheets for experimental write-ups are included in the manual, as are also forms for that material which needs to be mineographed. A helpful list of firms which deal in laboratory equipment is appended. The syllabus should supply a real need as a bit of enlivenment for the all too customary textbook course in psychology.

DePauw University STEPHEN M. COREY

Educational Measurement in the Elementary Grades. By I. N. MADSEN. Yonkers, World Book Co., 1930. Pp. x, 294.

This is a useful rather than a highly important or significant book. Like certain men, it is adequate without being brilliant—and adequacy, though less exciting, is often more satisfying.

Recognizing the needs and limitations of beginning students in normal schools and colleges, the author has attempted to meet these needs without putting a strain upon the limitations. Assuming a complete lack of information on the part of the student in the field of tests and measurements, he devotes the first part of his book to a simple presentation of the facts of individual differences and of elementary statistical procedures.

A chapter each is given to individual intelligence tests and to group tests. This material is followed by the most distinctive section of the book—a presentation and brief discussion of all the worth-while achievement tests on the elementary school level. Any teacher, beginner or otherwise, would find this material of great practical value. As Terman says in the Editor's Introduction, "Dr. Madsen's book gives about all the information on this subject that can reasonably be regarded as essential for the rank and file of teachers to have."

University of Minnesota HERBERT A. CARROLL

"Insight" in Rats. By E. C. TOLMAN and C. H. HONZIK. University of California Publications in Psychology, Vol. 4, No. 14. Berkeley, University of California Press, 1930. Pp. 215-232.

This brief paper reports three attempts to prove "insight" in the white rat in various types of maze situation. The general procedure involved, first, a preliminary training on the apparatus in which a strong preference for one pathway to food was developed, a less strong preference for a second pathway, and no preference for a third pathway; secondly, the "insight" runs in which only the latter pathway is open. If this pathway is taken directly by the rat, then insight is supposedly indicated. Tests were made on three types of mazes, embodying this essential feature but different in minor details. In only one of the three set-ups were positive results secured. The authors acknowledge that we do not know under what conditions insight appears, and they might have added that we do not know what insight is, either in rat or man. Interpretations aside, the authors have shown how the white rat behaves in three situations in which it looked as if "insight" should appear if the rat possessed the same sort of insight as the authors exhibited in arranging the problem for the rats.

Columbia University C. J. WARDEN

Coöperation and Competition: An Experimental Study in Motivation. By JULIUS BERNARD MALLER. New York, Bureau of Publications, Columbia University, 1929. Pp. x, 176.

Eight hundred and fourteen pupils of grades V to VIII were tested for speed of simple addition (1) when merely practicing, (2) when attempting to attain high individual ranks, and (3) when the scores of one school class were to be compared with those of another class. The difference between the pupil's score when work-

ing for himself and his score when working for the group was taken as a measure of his coöperativeness. A second measure of coöperativeness was obtained by allowing the pupils to choose at the beginning of seven tests whether they would work for themselves or for the group. Competition (work for self) was found to result in greater speed than coöperation, 32.4 examples more on the average in 12 minutes work, a difference 13.2 times the S.D. Also work for self was chosen 74% of the time. The two measures of coöperativeness were found to have a correlation of 0.23, although the reliability of each measure was found to be high.

Coöperativeness was found to differ with the homogeneity of the group (work in a specially organized team was more efficient even than work for self), with sex (girls more often choose to work for the group, but in general they do not work as near their maximum speed when working for the group), with nationality, and with the amount of discussion about the significance of coöperation (Boy Scouts, Girl Scouts, etc. were more coöperative than their classmates, and after a series of three discussions the work for the group was more efficient). Siblings and mutual friends were found to resemble each other in coöperativeness.

The measures of coöperation were correlated with a great number of other measures and ratings: (1) rating in coöperativeness by classmates and teachers—.35, (2) rating in coöperativeness by a psychologist—.80, (3) other tests of helpfulness—.42, (4) intelligence—positive but curvilinear, (5) chronological age, mental age, moral knowledge, resistence to suggestibility, deportment in school, character ratings, tests of honesty and inhibition, neurotic index—positive, (6) speed of work, variability in speed, health condition, vision, condition of teeth, nutrition, socio-economic status, frequency of attendance at movies, weight-height ratio—negative, (7) number of friends and number of siblings—positive but curvilinear.

Those who were in the upper quartile of coöperativeness as measured by the difference in speed of work for self and for the group were compared on 66 variables with those in the lower quartile.

This study is, in the reviewer's opinion, an important contribution in the field of character education and trait measurement. The low correlation between the two measures of coöperativeness and between these and other tests of helpfulness raises the question, pertaining to most tests. What is measured? But testing and correlating test results with other variables seems a promising method of attacking certain very real and practical problems.

Stephens College CARL N. REXROAD

Sangren Information Tests for Young Children. By PAUL V. SANGREN. Yonkers, World Book Co., 1930. Pp. iii, 28.

These are individual tests intended (1) "to call attention to the need for greater definiteness and certainty in nursery and kindergarten instruction," (2) to determine what young children at various stages of maturity and of instruction actually know, (3) to fix certain standards of promotion and placement, and (4) "to provide a basis for instructional practices and classroom management at the beginning of the first grade" (p. 1). Six types of material are used: Nature Study (28 items, maximum score 35); Numbers (17 items, maximum score 35); Vocabulary (35 words from the Gates Primary Reading Vocabulary, maximum score 35); Social and Civic Information (30 items, maximum score 31); House-

hold Utensils (16 items, maximum score 22); Language and Literature (eight divisions of items, maximum score 35). Children are examined individually, and the test scores are designed to supplement rather than to supplant intelligence measures. Norms (median scores) for the test as a whole and for the various parts are given for ages 5, 6, and 7 yr., and are based on the results secured with 91, 127, and 32 children, respectively. In the absence of more desirable criteria of validity, test scores were correlated (1) with chronological age—r ranged from 0.02 to 0.25; (2) with Stanford-Binet MA—r ranged from 0.46 to 0.73; and (3) with Stanford-Binet IQ—r ranged from 0.36 to 0.66. Reliability of the test obtained by correlating test-halves and using the Spearman up-step formula was 0.96.

A carefully planned and easily understood manual gives all instructions for scoring and recording. Publication, however, seems to have been premature as age-norms are based on data secured from only 250 children representing "not unselected groups" (p. 4), and standards of grade placement and promotion are derived from data for only 62 kindergarten and 63 first-grade children. One may question, moreover, whether the six types of material which comprise the tests furnish an adequate sample of a child's general information. The value of the tests for determining promotion and placement in the kindergarten and first grade is probably limited since the information tested is of little importance for the educational objective at this level of instruction.

Duke University W. A. BROWNELL

Reëducation psychologique par le travail. By MAURICE DIDE. Toulouse, Lyon & Bros., 1930. Pp. 22, four plates.

This is a pamphlet presenting from extensive successful experience a plea for the value of absorbing, constructive work in the psychological reëducation of mentally deranged individuals who have a certain degree of clearness. The author shows a number of full tone photographs of beautiful furniture, of decorative work forged in iron made in his own institution by the inmates, and of buildings constructed with their aid. The idea seems to be to get the inmates so interested in some real productive work of any kind whatever—including gardening, breeding domestic animals, work in nurseries, etc.—as a mode of self-expression that they lose the abnormal inhibitions and thoughts of self, and emerge from the monotony of routine existence and conflict into self-functioning individuals able to resume normal social habits. He admits that the surprise provoked by such tendencies to industrialism in mental hospitals indicates how such an innovation affects traditional opinions and conflicts with administrative dogmas. J. P.

Mind and the World-Order: Outline of a Theory of Knowledge. By CLARENCE IRVING LEWIS. New York, Charles Scribner's Sons, 1929. Pp. xiv, 440.

This is an approach to the problem of knowledge from the field of exact logic, from a point of view labelled by the author "conceptualistic pragmatism." The method is reflective; "it accords with the Socratic presumption that the truth which is sought is already implicit in the mind which seeks it, and needs only to be elicited and brought to clear expression. It accords, further, in the recog-

nition that it is definitions or 'essences' which are the philosophic goal." Lewis suggests that Reality (with a capital R) is a sterile concept. Instead, the term "real" can be compared to the term "useful," and as a thing may be useful for one purpose and useless for another, so it is real from one point of view and unreal from another. "Nothing is real in all categories; everything is real in some category." The categories are independent of experience in that they are derived by definition, and a thing may be unreal physically by definition, while being real experimentally. The proper task of philosophy is the consideration of these categories and the explicit definition of them.

A logical analysis is made of the knowledge process, and the conclusion is reached that "there is no knowledge of external reality without the anticipation of future experience." Knowledge, when valid, is always *verifiable;* verification may be actual or hypothetical.

The author criticizes, in passing, those who deny the existence of consciousness because the "mental" must always be analyzed in terms of bodily behavior. An analysis must interpret the thing analyzed in terms of its constant relation to other things, and a term *is not* another term with which it has a constant correlation. The book is primarily an essay in epistemology and metaphysic, and is not directly relevant to psychology. It demands and will repay careful study by those who are interested in the problems considered.

Cornell University MERRILL F. ROFF

La répression mentale. By PAUL SOLLIER. Paris, Félix Alcan, 1930. Pp. ii, 218.

Sollier is interested primarily in repression as a dynamic phenomenon, considering that the process of repression is more important than the thing repressed. He objects to the Freudian doctrine on two grounds: first, that there are no instincts, properly speaking, but imbalances in the organism; and secondly, that anything, good or bad, may be repressed if there is a contrary force working against it. "We may say, in effect, that there exists no psychological phenomenon having any more than another phenomenon the privilege of provoking mental repression."

Repression, for him, is a "phenomenon constituted by three essential and contemporaneous elements: a *tendency* arrested in its course; a *resistance* which not only arrests but also represses the tendency; and *affective reactions* or painful affective forces which result therefrom." The tendency is the activity of the organism in disequilibrium; the resistance may be internal or external, but is always active; the affective reactions are emotional disturbances. "Mental repression is essentially an active, dynamic process, but one in which the energy expended is employed in negative rather than positive work." Thus, the repressed individual may appear superficially inactive, but show definite fatigue; or he may even break under the strain, just as a ship may seem stationary but be strained by being impelled in one direction by the current and in the opposite direction by the wind. "Between the physico-chemical world and the psychic world there is no heterogeneity, and the same laws govern both."

Sollier finds the phenomena of repression in various aspects of life. All repression, of course, is not to be considered harmful, for our social life would be

impossible unless each individual were taught to give up certain personal liberties to ensure liberty to the group. The author views individuals as dynamic mechanisms amenable to the laws of energy-changes and the resultants of opposing forces just as are objects in the physical world. While such analogies are probably more helpful than to consider individuals as mystic compounds affected by such intangibles as "will," "subconscious memories," "the censor," and so on, no data are offered to support these analogies. *La répression mentale* is essentially a book of philosophical speculations.

University of Pittsburgh J. H. McFADDEN

The Principles of Reasoning. By DANIEL S. ROBINSON. Second Edition. New York, D. Appleton & Co., 1930. Pp. xviii, 393.

The modifications in the second revision consist in the addition of but little new material here and there and in minor corrections. References are brought up to date and new sets of exercises replace those in the first edition. The addition most interesting to the psychologist is a three-page discussion in which the author's 'implicative system' is identified with the pattern concept of *Gestalt* psychology. Three of Köhler's experiments "to determine the extent to which chimpanzees can reason" are considered. In what the author calls the *integrative experiment*, the problem for the chimpanzee was to complete a pattern which lacks but one link, as taking a stick or other long object to pull a banana within reach; in the *analysis experiment* the animal was to pull a banana within reach by one of two available strings so arranged that the objective would have to be pushed back and to the side behind a wooden frame and then pulled in, rather than being pulled in directly by the other string; and in the *synthesis experiment* two sticks put inside the cage were to be joined together like a jointed fishing pole so that the food could be reached. Robinson contends that in these experiments the solution depended on the animal's "comprehending the way the pattern is to be completed before he performs the action to complete it" (p. 11). This method of simply reasoning from examples of problems solved to the conclusion that certain pattern (or other) relations were apprehended now appears naïve to the student of animal behaviour. It is essentially the method that was early discarded in experiments with animals, for good empirical reasons. The author is not unaware that explanations in terms of purpose are objectionable to science, but holds rather arbitrarily to the need of such explanations in "ethics, legal science, and other social sciences" (p. 322). This persistence in old methods (which have proved inadequate *per se* in experimental research) is worth serious consideration in relation to the relative lack of progress in ethics and 'legal science.' Action with purpose is certainly not characterized by non-predictability or lack of relation to external and bodily conditions. J. P.

The Meaning of Sacrifice. By R. MONEY-KYRLE. The International Psycho-Analytical Library, No. 16. London, Hogarth Press, 1930. Pp. 273.

This treatise is a contribution to the Freudian literature, attempting "to apply the findings of psychoanalysis in the social field" (p. 9). Its primary thesis is "that sacrifice is a symbolic parricide" (p. 207), a "neurotic symptom" (p. 71).

Incestuous and parricidal desires are assumed, numerous inhibitions compel repression of them, and there result an ambivalence of love and unconscious hate producing a sense of impotence, until by a process of projection and substitution the original parricidal desires are directed toward a chief, a totem, or a god, "the symbolic expression of an unconscious desire for parricide which each individual has acquired for himself" (p. 260). Distribution, modes, and theories of sacrifice are discussed. An exhaustive index is provided, but no bibliography although frequent references are given in footnotes.

This book presents an excellent illustration of the extent to which a favorite theory may be applied by an uncritical enthusiast. It abounds in assumptions. Probably its chief value is that it contributes an elaboration of a single Freudian principle, thus furthering its ultimate acceptance or rejection.

Syracuse University ERNST THELIN

Value of Marking Hard Spots in Spelling. University of Iowa Studies in Education, Vol. V, No. 4. By L. S. TIREMAN. Iowa City, The University Press, 1930. Pp. 48.

For the study of the influence of marking hard spots in learning to spell, two lists of words were selected from Master's lists of words. The hard spots were indicated to the experimental group by (1) underlining, (2) using capital letters, (3) using bold faced type, or (4) having the child mark his own hard spots. The only other difference in method was this statement to the group mentioned: "Note that a part of each word on the study is marked in a special way. This indicates that it is a hard spot in the word—one that you are apt to misspell." The conclusions are: "After a study of over 4000 pupils of the 4th, 6th, and 8th grades, involving a half million spellings, one is impressed with the consistency with which the data show that marking hard spots is of little or no value. . . . The fact stands out that the pupils who studied words with the hard spots marked made poorer scores than those who studied lists with words unmarked."

University of North Carolina A. M. JORDAN

Non-Language Primary Mental Test for Kindergarten and Grades I and II. By RUDOLF PINTNER. New York, Teachers College Bureau of Publications, Test Booklet, 1929, pp. 20. Manual of Directions, 1930, pp. 11.

This test is devised for young children in the kindergarten and first two grades. All the directions are to be given in pantomime in the form of a game. Though the author says that the test is difficult to give, the procedure is outlined in clear detail and the test material is simple, easily handled, and interesting.

The test, which might more accurately be called a scale, is made up of four sub-tests including a total of 83 separate test-items. Directions are given for weighting scores on the separate sub-tests, and a table presents norms for changing total weighted scores into years and months of mental age. Norms are based upon responses from 157 children at kindergarten level, 123 children from grade I, and 168 children from grade II.

The reliability of the test, calculated by correlating odd and even items, is given as 0.90 for the first grade (N = 111), and 0.86 for the second grade (N = 129). It is not given for the kindergarten children. Kindergarten scores correlated with Stanford Binet scores (on 80 *Ss*) gave a coefficient of 0.61, and 154 kindergarten and first and second grade scores yielded a correlation of 0.51 with the Pintner-Cunningham test.

Child Guidance Clinic, St. Paul, Minn. GRACE ARTHUR

Test Method Helps, No. 2. Manual for Determining the Equivalence of Mental Ages Obtained from Group Intelligence Tests. By ROSS O. RUNNELS. Yonkers, World Book Co., 1930. Pp. 14.

It is well known that there are enormous variations in mental ages as determined by different group tests. This *Manual* gives "equivalent mental-age norms" worked out for the following group tests: National Intelligence Tests; Haggerty Intelligence Examination, Delta 2; Otis Group Intelligence Scale, Advanced; Terman Group Test of Mental Ability; and Otis Self-Administering Test, Higher. The first forms of these tests were used, but with exceptions noted, the forms may be disregarded. These norms were obtained from the scores of one application of each of the tests to the same 1422 5th to 9th grade pupils within a single school year, practice effects of the several tests being experimentally controlled. Percentile scores were found and graphs made for each test by interpolation, and the average of the mental ages in the five tests for corresponding percentile scores were called composite mental ages. For example, the composite mental age of the 50 percentile (median) was found by averaging the mental age scores of the several tests as found from their own norms in order as named above. Thus 14-7, 15-0, 15-9, etc., gave a mean of 14 years 10 months. This mental age is therefore given in the table opposite the 50 percentile scores of the several tests; and so on, for the other percentile scores. If the IQ is to be determined it is suggested that the formula IQ = MA/CA be used up to the age of 14 years, at which point Runnels supposes mental growth begins to slow up. After 14, a youth's IQ is to be determined by dividing the composite mental age by a purely hypothetical mental age given in the author's Table 3, which does not increase after the chronological age of 16 years. Suggestions for similar procedures with other tests than those used are given.

It may be noted that since the children were selected by grades, rather than by ages, those in the upper grades were probably superior samplings. An important research problem on mental growth by slightly improved but similar methods might be approached from an attitude that is more experimental than the practical one manifested in the construction of the *Manual*. Mental ages above the periods of retarded mental growth might be checked against scores by groups of mature superior children at different IQ levels as determined by earlier tests at 12 years of age. In such a study reliability coefficients (unfortunately neglected here) should be carefully brought up to a very high point. Low reliabilities greatly and erroneously increase individual variations. J. P.

Mental Measurement of Preschool Children. By RACHEL STUTSMAN. Yonkers, World Book Co., 1931. Pp. x, 368.

The book is in four parts: (1) Problems and History of the Mental Testing of Preschool Children; (2) The Merrill-Palmer Research; (3) Guide for Administering the Merrill-Palmer Research; (4) Illustrative Case Studies. Of these, perhaps part 4 will be of more general interest to psychologists; while parts 2 and 3 will be of practical use to the clinical worker.

After summarizing the earlier work in the field, the author describes the Merrill-Palmer scale which is designed to utilize the successful features of other tests, and to avoid other features which have not proven valid. Special care was given, in selecting material, to the factors of inherent interest, variety, variation in difficulty, freedom from environmental influence, simplicity, ease of administering, objectivity, and the usual statistical evidences of validity and reliability. The tests are largely of the performance type, and the statistical work is unusually thorough.

The scale itself consists of 93 test items, ranked according to difficulty, representing a range of 18-71 mo. in mental development. One obtains the impression that clinical workers will have some difficulty in applying the directions for scoring and interpreting the tests although the procedure is explained in detail. While there has been marked improvement on Gesell's method, of which the author is properly critical, the scale would seem to be capable of much greater usefulness if it could be improved in this important respect. The statistical data might more appropriately have been issued as a technical monograph.

Notwithstanding the difficulties pointed out, the author has in the opinion of the reviewer the best available method for measuring the mental development of young children.

University of California at Los Angeles J. HAROLD WILLIAMS

The Field of Philosophy: An Introduction to the Study of Philosophy. By JOSEPH A. LEIGHTON. 4th Edition. New York, D. Appleton & Co., 1930. Pp. xii, 639.

The chief additions and modifications in this revision bearing on psychology are the new chapter 17, Theories of the Soul or Mind, and some new material on dualism and mind-body relations in chapters 13, 14, and 16. J. P.

An Anthology of Modern Philosophy: Selections for Beginners from the Writings of the Greatest Philosophers from 1500 to 1900. By DANIEL S. ROBINSON. New York, Thomas Y. Crowell Co., 1931. Pp. xiii, 836.

As in the author's *Anthology of Recent Philosophy*, this volume includes analyses before each selection, and at the end of each chapter it gives suggestions and questions for discussion. A biographical sketch of each author quoted, together with references, is given in the appendix. The present volume begins with selections from Paracelsus and ends with T. H. Green. There appear for the first time a number of translations of these masters of thought, and certain rare selections now out of print and mostly inaccessible to students are included. The book will be appreciated by persons who are interested in historical phases of psychology as well as by philosophers and general readers. J. P.

Mahatma Gandhi's Ideas: Including Selections from His Writings. By C. F. ANDREWS. New York, Macmillan Co., 1930. Pp. 382.

This is not a scientific book. While it attempts to present and evaluate Gandhi's views and something of the complicated religious and race difficulties in India, it is inclined to evaluate them from a particular (the Christian) point of view, even though it represents Gandhi as having a humanitarian viewpoint that is more comprehensive than that of any religion. J. P.

Personality: The Crux of Social Intercourse. By A. A. ROBACK. Cambridge, Mass., Sci-Art Publishers, 1931. Pp. 144.

This little volume is an expansion, for elementary students in psychology and intelligent lay readers, of the author's article of the same title published in the *Century*, 1929. It is intended as "a summary of the work and a description of the methods employed recently in the study of personality" (p. 7), but as a matter of fact very little is said anywhere of scientific method. The book is wholly qualitative and contains nothing new to psychologists, either as to content or as to point of view; and no such contributions are intended. More advanced students are referred by the author to his larger volume, *The Psychology of Character*, 1927. There is a bibliography of 128 titles. J. P.

Psychological Service for School Problems. By GERTRUDE H. HILDRETH. Yonkers, World Book Co., 1930. Pp. xiii, 317.

This book describes the applications of educational psychology to the problems of the school. It is more concerned with the situations in which psychological service is demanded than with the fundamental principles of educational psychology. The principles and technique of psychological measurement are, however, described in some detail. The study, classification, and treatment of exceptional pupils are described primarily as problems of the school, but in the light of psychological principles.

The chief value of the book lies not so much in the information it gives to the students of psychology, as in the psychological approach it gives to the student of school problems.

Duke University HOWARD EASLEY

Exploration psychologique infantile par l'expression graphique du mouvement. By MAURICE DIDE and MATHILDE GUILHEM. Extrait des Memoires de L'Academie des Sciences, Inscriptions et Belles-Lettres de Toulouse, Douzieme Serie, Tome 7, pp. 247-259. Toulouse, J. Bonnet, 1929.

The authors report here their investigation of the development of the ability of graphic representation in the young child. The investigation includes their observations of some 50 children over a period of several years. A number of situations, and some typical reactions to them, are described, such as: (1) Pseudo-schematization of attitudes. Here the child is asked to assume a position, *e.g.* arms extended, and then to draw immediately what he has done. Representations in this class include merely direction and approximate spatial relation of gross features. (2) Pseudo-schematization of movement. Here the

child is asked to execute some gross movement and then draw it. The responses indicate simply direction of movement, and do not represent parts of the body as moving. (3) Cinematographic representation of movement. Here the drawings illustrate movements by drawing the moving members in different positions. These three stages are illustrated with description of typical cases, and also with some original drawings of the children.

The conclusions concerning the developmental relation of these processes to various mental processes seem to be based upon the whole of the observations, only a few of which are reported. Enough are reported, however, to indicate the significance of the method of investigation.

Duke University HOWARD EASLEY

Statistical Tables for Students of Education and Psychology. By CARL J. HOLZINGER. Third Edition. Chicago, University of Chicago Press, 1931. Pp. viii, 101.

In the present edition Table XX is added to the tables found in the second revision (1928). This new table covers pages 82-101, and facilitates the calculation of partial correlations by giving the values of the denominator $[(1-r^2_{13})(1-r^2_{23})]^{-\frac{1}{2}}$. Numerator values like $(r_{12}-r_{13}r_{23})$ are worked out arithmetically to three decimal places and are then multipled by the denominator value which is read off directly from the table. Table XX covers all values for the two correlations indicated from 0.00 to 0.99. By using r values that are correct to three decimal places similar values for the partial correlations may be interpolated from Table XX. With the additional use of the Crelle *Calculating Tables*, partial correlations may be worked out with considerable speed.

J. P.

The Book of My Life (De vita propria liber). By GIROLAMA CARDANO. Translated from the Latin by Jean Stoner. New York, E. P. Dutton & Co., 1930. Pp. xvi, 331.

This book is an English version of the autobiography of the celebrated Milanese physician, mathematician, and scientist. Cardan was a contemporary of Cellini and a friend of Vesalius. However he knew very little about anatomy and surgery, but was sympathetic with the struggles of the great pioneer of race craniology (Vesalius) and once refused an offer to work with him.

The volume is of special interest to psychologists in that we find in the author such an unusual combination of personality traits—a scientist, who is at the same time morbid and superstitious, extremely sensitive and famished for fame, yet withal remarkably honest and self searching. At times he was something of a paranoiac, frequently having persecutory and grandiose delusions of a more or less mild degree. But in spite of these eccentricities (and his ignorance of anatomy), Cardan was deeply interested in the brain and nervous system, and their relation to physical and intellectual life. He was perhaps the first man to sit in clinic and ponder his own morbid experiences—he was psychiatrist and patient in one.

The book will probably rank as one of the few great autobiographies in literature. The translation is excellent and the notes very helpful. A bibliography of thirty-two references is given.

University of South Carolina W. F. SMITH

Les trois ages de la science. By BARON DESCAMPS. Extrait des Bulletins de la Classe des Letters et des Sciences morales et politiques. 5e serie, 1. xvi, No. 5. Bruxelles, Marcel Hayez, 1930. Pp. 257-273.

This is a lecture given by the author before the Royal Academy of Belgium. The first half of the discussion is devoted to a critical evaluation of the concepts of Descartes, Kant, and Auguste Comte, and the effects of these concepts on scientific progress. The three ages of science are designated as (1) the age of hegemonic concentration, (2) the age of autonomous separation, and (3) the age of harmonious expansion. The first age is characteristic of the past, the second of the present, and the third of the future. As a psychological inquiry the paper has relatively little value from an empirical point of view. It is of value, however, as a philosophical orientation to Bergsonian metaphysics.

University of South Carolina — W. F. SMITH

L'ologénèse: Nouvelle théorie de l'évolution et de la distribution géographique des êtres vivants. By DANIEL ROSA. Paris, Félix Alcan, 1931. Pp. ii, 368.

In this volume Rosa, of Florence, proposes a new theory of evolution which is also suggested to account for the geographical distribution of living forms. According to the view proposed in the book every species is predetermined in preceding species, as each individual is predetermined in the egg from which it develops. The book deals successively with an explicit and detailed statement of this theory and of the relationship of the theory to other theories of evolution and also to various evolutionary concepts such as that of regression.

The reviewer finds in this volume little of direct or inferential value to the study of the inheritance of psychological characteristics.

Brown University — LEONARD CARMICHAEL

Test Method Helps No. 3. Expressing Educational Measures as Percentile Ranks. By FRANCIS C. BUROS and OSCAR K. BUROS. Yonkers, World Book Co., 1930. Pp. 30.

In obtaining the percentile rank this method requires that all the cases be ranked and then the ranks looked up in a table under the appropriate number of cases. The percentile rank of an individual is taken to be the mid-point of the interval in which he appears, thus with ten cases the highest percentile rank is taken at 95. For practical purposes a simpler way to compute percentile ranks is to construct an accumulative frequency table, obtain the reciprocal of one case to three decimals, set Crelle's Rechentafeln at this number, and read off the percentages for each interval.

University of North Carolina — A. M. JORDAN

Instructional Tests in Chemistry. By EARL R. GLENN and LOUIS E. WELTON. Yonkers, World Book Co., 1930. Pp. iv, 76.

A comprehensive set of questions has been prepared on 36 different topics in Elementary Chemistry. The items under each topic vary in number from 18, which consist largely of problems, to 80 which consist of writing symbols and formulas. Reliabilities furnished for each topic range from 0.70 to 0.88. Norms for each fifth percentile are published for each of the 36 tests.

University of North Carolina — A. M. JORDAN

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By J. P. GUILFORD, University of Nebraska

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